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BIOLOGY AND PHYSICS*

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I HAVE been asked to take part in a discussion in which biology is to be compared with physics. But as I am too ignorant of modern physics to do this I must confine myself chiefly to stating what appear to me to be the most general peculiarities of biology. At the same time I shall presuppose no knowledge of biology on the part of my audience beyond that which is inevitably picked up in the course of everyday life. I shall try to show that the features which I have selected for mention are all traceable to a single general principle which can be understood by any one. At the same time, as I am not writing an article for the *Encyclopedia Britannica*, I shall make no claim to cover the whole subject. For example I am not at all sure that the remarks that follow apply to bacteria or to viruses.

What I am going to call the basic principle of biology is the statement which asserts that living things have *parts* which stand in the relation of *existential dependence* to one another. What is meant by this will, I hope, be clear from the following examples. When a man's head is cut off the head and the trunk both perish. Head and trunk are mutually existentially dependent. When a man's leg is cut off the leg will perish and so will the trunk if bleeding does not stop in time or the wound becomes fatally infected. The leg is existentially dependent upon the trunk but the trunk is not so dependent upon the leg. An example on the social level is provided by the existential dependence of a new born infant on its mother. It must not be supposed that existential dependence ceases to hold when a substitute for the normal second term of the relation can be found. An infant does not cease to be existentially dependent because the duties of its mother are taken over by a foster-parent. Similarly, parts which can be kept alive in

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some artificial medium do not cease thereby to be existentially dependent; in this case the biologist who provides the artificial medium, and other care, is the analogue of the foster-parent; so is the surgeon who, in the case of the amputated leg, stops bleeding and takes anti-septic precautions. Dependence of roots on leaves and leaves on roots provides an example from the plant world. The study of the mutual inter-dependence of parts and of parts on environmental factors constitutes the task of one of the primary subdivisions of biology, namely physiology.

I turn now to a class of relations which I call hierarchy-generating relations which also appear to be highly characteristic of biology, and the exemplifications of which are connected with the occurrence of existential dependence between parts. But before I can explain what I mean by a hierarchy-generating relation I must first explain what I mean by a hierarchy. And by a hierarchy I mean any relation which is one-many and such that its converse domain is identical with the whole set of terms to which the beginner of the relation stands in some power of the relation. This is a purely abstract definition because the notion of hierarchy as used here is not one belonging to any particular empirical science. It is a purely set-theoretical notion. At the same time it does not occur in those sections of Whitehead and Russell's *Principia Mathematica* which are devoted to the theory of relations. Nevertheless a class of relations is defined there which is a *sub-class* of the class of all hierarchies, namely the class of *progressions*. A progression is a relation which is one-one (i.e. many-one as well as one-many) and such that its *domain* consists of its beginner and all the terms to which that beginner stands in some power of the relation. A progression is thus a hierarchy which is many-one and has no last term. A relation which differs from a progression only in having a last term may be called, in relation theory, a *line*. It is most neatly definable as a hierarchy whose converse is also a hierarchy.¹

To illustrate these general remarks and especially to emphasise what is excluded by the notion of hierarchy I give some arrow-figures. And first I give (Fig. 1) a diagram of domain, converse domain, and field of a relation to illustrate some words I shall use which are not used in *Principia Mathematica*. Members of the field of a relation which do not belong to its converse domain are called the *beginners* of the relation in *Principia Mathematica*. In the case of a

¹ I have used this notion in *Biology and Language*, London, 1952, p. 222. I owe the above improved definition to Dr A. Lindenmayer.

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hierarchy this class has only one member. Members which belong to the converse domain but not to the domain are there called *beginners* of the converse, but I shall call them the *terminals* of the relation.

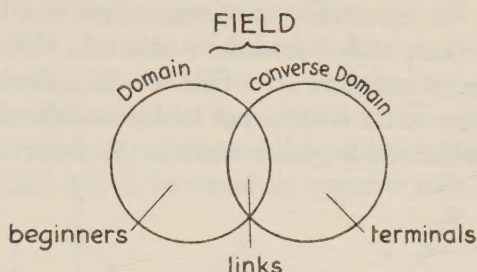


FIG. 1. Diagram of the three mutually exclusive subsets of the field of a two-termed relation: *beginners*, things which stand in the relation to something but have nothing standing in the relation to them; *terminals*, things which have something standing in the relation to them, but do not stand in it to anything themselves; *links*, things which both stand in the relation to something and have something standing in it to them.

Members of the field which belong both to the domain and to the converse domain I shall call *links* of the relation. In an arrow-figure beginners will have an arrow running from them but not to them, terminals will have an arrow running to them but not from them, and links will have at least one arrow running to them and at least one running from them. A diagram of a regular and one of an irregular hierarchy are given (Fig. 2); the former is such that every member of

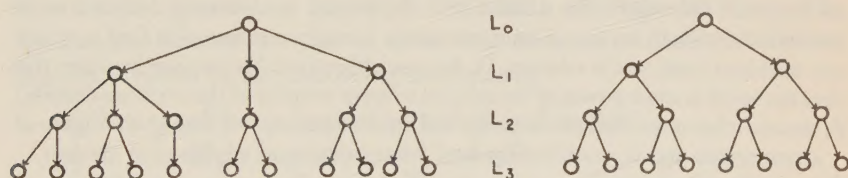


FIG. 2. Arrow-figures of hierarchies; irregular on the left, regular on the right. Numbering of levels indicated by L_0 , L_1 , etc.

the domain stands in the relation to exactly the same number of members of the converse domain as every other member. Arrow-figures of relations which are *excluded* by the definition of hierarchy are also given (Fig. 3): thus A is not a hierarchy because it has no beginner although it has a finite field. B has no beginner because its domain is included in its converse domain; the dots are intended to indicate an

infinite regress. C is not a hierarchy because it is not one-many. D is not a hierarchy because, although it has one and only one beginner, this beginner does not stand in some power of the relation to every member of the converse domain. E is not a hierarchy because it has two beginners. F represents a progression and G a line. A useful notion in connection with hierarchies is that of a *level*. A level of a hierarchy is a set of members of its field to which the beginner stands in the same power of the relation and which includes all the members of the field to which the beginner stands in this power of the relation.

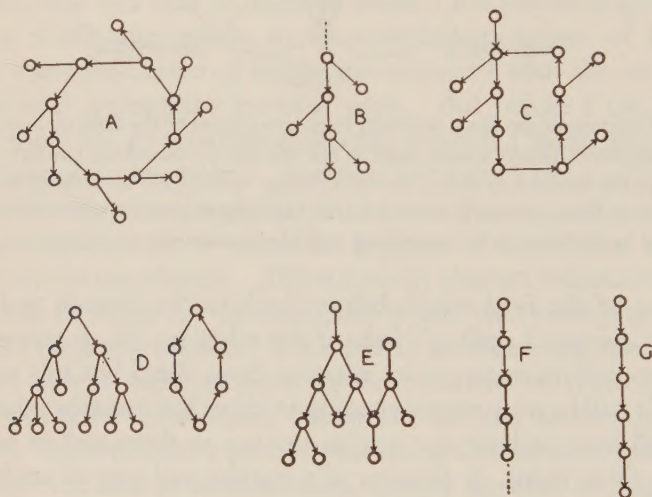


FIG. 3. A to E arrow-figures of relations which are *not* hierarchies: A, because it has no beginner, although it has a finite field; B, because its domain is included in its converse domain; C, because it is not one-many, having a member of its field to which *two* members stand in the relation; D, because although it has but one beginner this does not stand in some power of the relation to *every* member of the converse domain; E, because it has more than one beginner *and* is not one-many. F is an arrow-figure of a progression and G an arrow-figure of a line in the sense explained in the text.

Thus the beginner itself is the sole member of zero level; the terms to which it stands in the relation constitute level one; the terms to which they stand in the relation form level two, and so on.¹

We can now proceed to hierarchy-generating relations. A relation is hierarchy-generating if and only if by taking any member

¹ For further information about hierarchies and for explanations of technical terms belonging to relation theory which have been used above see the author's *Axiomatic Method in Biology*, 1937. For a good explanation of the notion of being related by some power of a relation see Quine's *Mathematical Logic*, 1951, p. 215.

of its domain and limiting its field to that member and all the terms to which it stands in some power of the relation, the relation so limited satisfies the definition of hierarchy. Thus if the story of Adam in the book of Genesis is accepted as quite literally true then the relation of fatherhood is a hierarchy because there will be one and only one father who is not a child. But even if fatherhood is not a hierarchy it is a hierarchy-generating relation. For if we take any man who has children and limit the field of fatherhood to this man and all his descendants, the resulting relation will be a hierarchy. The female members of its field will all be terminals, together with all the childless males.

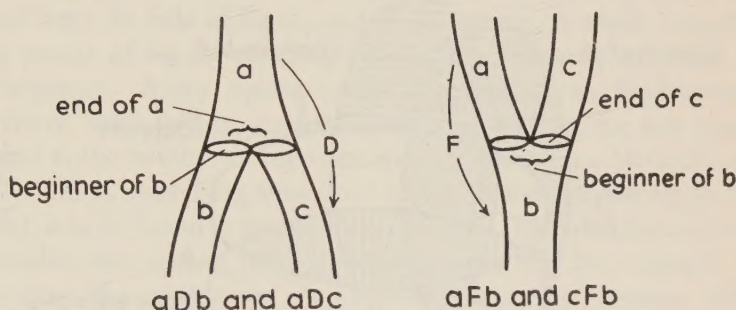


FIG. 4. Space-time diagram of the modes of origin of cells, depicting cells related by **D** and **F**.

Examples of hierarchy-generating relations in biology (apart from the one just given) are provided by certain relations between *cells*. All that it is necessary to know about cells in order to understand what follows is given in the following statements:

- (1) Every cell has a beginning and an end in time;
- (2) In no cell do beginning and end coincide;
- (3) The beginning of every cell *either* (i) is a proper part of the end of a previously existing cell, *or* (ii) is the result of the *fusion* of the ends of *two* cells.
- (4) Every cell possesses existentially dependent parts.
- (5) Every cell has an environment.

Let us denote by '**D**' the relation in which a cell *a* stands to a cell *b* (see Fig. 4) when the beginning of *b* is a proper part of the end of *a*. And let us denote by '**F**' the relation in which a cell *a* stands to a cell *b* when the end of *a* is a proper part of the beginning of *b*. Then

we can say that every cell belongs to the domain or the converse domain of at least one of these relations (see Fig. 5) and that **D** and the converse of **F** (which can be denoted by ' \mathbf{F}^{-1} ') are hierarchy-generating relations. These two relations differ enormously in the size of their fields. The number of links of **D** and of terminals of **D** is colossal and so are the powers of **D** which exist. But links of **F** are comparatively rare; they occur in the embryo sacs of flowering plants but apparently nowhere else; and powers of **F** above \mathbf{F}^2 are empty. The importance of **F** seems to lie in the way in which it provides a means whereby terminals belonging to two **D**-hierarchies may unite to form the beginner of another.

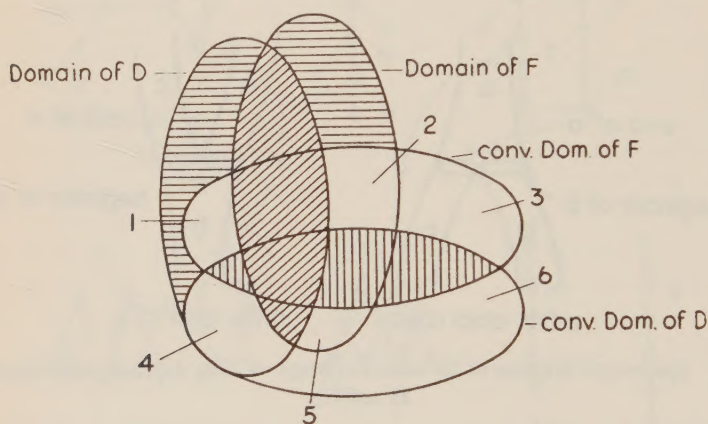


FIG. 5. Diagram of the constituents of the union of the fields of **D** and **F**. Sets having no members are represented by shaded areas. Non-empty sets: (1) beginners of **D** which arise by fusion (called zygotes in animals); (2) links of **F** (fused polar cells in the embryo-sacs of flowering plants); (3) terminals of **F** (in animals, zygotes which fail to divide); (4) links of **D**; (5) beginners of **F** (gametes in animals); (6) terminals of **D** other than gametes.

It is not difficult to see a connection between existential dependence and the occurrence of the **D**-relation between cells. If you have a bicycle and you want another but cannot get parts to put together to form one you have no recourse but to saw your bicycle into two halves—splitting each wheel, bar, pedal, etc., longitudinally. If cells have existentially dependent parts new cells will not begin by the coming together of parts but by the splitting of parts of existing cells. Cells are tender things and would have disappeared long ago had they not possessed the power of division. But division alone will not suffice—it would merely yield smaller and smaller cells. We should expect

division to be preceded by a process of *duplication* of parts within cells from raw materials *ingested* by the cell from its environment.¹

Now we must turn to another hierarchy-generating relation among biological objects. Consider first a square. If we bisect each side and join opposite bisecting points by straight lines we have four smaller squares which together compose the square with which we began. The same process can now be repeated with each of the four smaller squares producing sixteen still smaller squares, and so on. Let us denote by '**Sq**' the two-termed relation between any square and each of the four smaller squares into which it is divisible in the above way. Then we can say that, although **Sq** is not a hierarchy it is a hierarchy-generating relation. For, if we take any square—call it *s*—and limit the field of **Sq** to *s* and all the squares to which *s* stands in some power of **Sq** the resulting relation is a hierarchy of which *s* is the beginner. Every square is the beginner of an **Sq**-hierarchy. Moreover, these will be regular hierarchies; because for any natural number *n*, the number of members in level *n* of such a hierarchy will be 4^n . Now, something analogous to this, but much less simple and regular, is to be found in the biological world. The adult human body is divisible into certain parts (called *organ-systems*), for example, the alimentary, the respiratory, the excretory, and other systems, which collectively and in their specific and very complicated anatomical relations compose the body. Each organ in turn is analysable into certain parts (organs), the alimentary system, for example, is analysable into such parts as mouth, pharynx, gullet, stomach, intestine, liver, pancreas, which together in their anatomical relations compose it. Each organ again is resolvable into certain regions or layers which exemplify what are called *tissues*. Thus the intestine has lining layer, a muscular layer, a nervous layer and a covering layer. But of these layers only the lining is specifically concerned with alimentation, the muscular layer belongs to the muscular system which here, so to speak, combines with the alimentary system to ensure movement of the food along the canal. Each tissue is composed of cells of a characteristic kind, together with (in some cases) certain cell-products like fibres or tissue fluids. Each cell is next analysable into two major

¹ The process of duplication prior to division is beautifully illustrated by the protozoan *Euglypha alveolata* which is covered by small shell-plates except for a small hole for the protrusion of pseudopodia. Prior to division a duplicate set of shell-plates is formed round the nucleus. See E. A. Minchin: *An Introduction to the Study of the Protozoa*, 1912, p. 112, Fig. 59.

parts: a central nucleus (some cells have more than one) surrounded by a part called the cytoplasm. Each nucleus is composed chiefly of chromosomes, although there are also non-chromosomal parts; and each cytoplasm has such parts as mitochondria, Golgi bodies, secretion granules, etc. And so on. Certain of the spatial parts of the human body thus fall into *levels* in a spatial hierarchy. Let us call the relation in which the whole body stands to each of its organ-systems or the relation in which an organ system stands to each of its organs or any part to each of its next-level parts (like the relation between square and each quarter-square) the relation **S**. Then although **S** is not a hierarchy each whole body will be the beginner of an **S**-hierarchy.

But instead of talking about bodies we shall speak of *lives* to remind ourselves that we are speaking about time-extended things. Some cells are lives and some are not lives, but the cells which are not lives are proper parts of lives which are not cells. Lives are objects to which *taxonomic names* are applicable, but to the spatial parts of lives such names are not applicable. Thus we say that a man is a member of the species *Homo sapiens*, but we do not say that his big toe or his liver is a member of this or any other taxonomic group. Consequently cells which are lives are also given taxonomic names, like *Amoeba proteus*, the microscopic life which is found in ponds. But cells which are spatial parts of lives which are not cells are not given such names. We must distinguish between unfinished lives and finished lives. A life *a* which is a proper part of another life *b* is an unfinished life if its end is before that of *b* in time. Thus a child of one year old which continues living until it is two years old is an unfinished life. A finished life is one whose end is the beginning of a *corpse*.

In addition to these remarks about lives it is also necessary, in order to understand what follows, to know what is given in the following statements:

- (1) Every life has a beginning and an end in time.
- (2) In no life is the beginning coincident with the end.
- (3) The beginning of every life which is not a cell is *either* (i) the beginning of a cell which is a life, *or*
(ii) a cluster of beginnings of cells which are not lives.
- (4) If the beginning of a life is the beginning of a cell that cell belongs to the converse domain either of **D** or of **F** (see Fig. 5).
- (5) If the beginning of a life *b* is the beginning of a cell belonging to the converse domain of **D**, then there is a life *a* which is not a cell

from which that cell has been derived by division; the life a is called the asexual parent of b (Fig. 6 (ii)).

- (6) If the beginning of a life c is the beginning of a cell c belonging to the converse domain of \mathbf{F} , then there is at least one life a which has produced a cell which has fused with another cell to form c , and a is called a *sexual* parent of the life c (and of any life of which c is a proper part). (Fig. 6 (iii)).

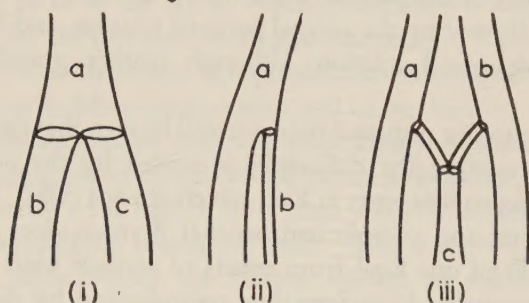


FIG. 6. The three modes of origin of lives which are not cells: (i) division; (ii) budding (if beginning with many cells), or parthenogenesis (if beginning with one cell); (iii) sexual reproduction. In (i) a is asexual parent of both b and c ; in (ii) a is asexual parent of b ; in (iii) a and b are both sexual parents of c ; a and b may be one, when both gametes are produced by the same life.

- (7) If the beginning of a life is a cluster of beginnings of cells which are not lives there are again two possibilities: (i) a life which is not a cell *divides*, i.e. it ends in *two* cell-clusters *each* of which is the beginning of a life which is not a cell. In this case the first life is asexual parent of *both* of the resulting lives (Fig. 6 (i)); (ii) a life which is not a cell separates off a cluster of beginnings of cells which forms the beginning of the new life but does not itself end until later. In this case also the first life is asexual parent of the second (Fig. 6 (ii)). In case (i) it is customary to speak of one life *dividing* to form two (just as with division of cells); in case (ii) we speak of the parental life *budding* to produce offspring (if the life b begins from a cluster of cells).
- (8) The cells which are parts of a life which is not a cell fall into at least two mutually exclusive classes such that the members of one class are existentially dependent upon members of the other classes.
- (9) Every life which is not a cell has an environment.

In connection with the different ways in which lives which are not cells may begin it may be of interest to mention that in what is

called identical twinning a combination of *two* processes is believed to occur. First a life begins as a result of a *sexual* process and then this life *divides* to form two lives—the identical twins—which are thus the immediate result of an *asexual* process. It is a consequence of this that (contrary to our usual way of speaking) the woman who gives birth to a pair of identical twins is *not* (biologically speaking) their mother but their *grandparent*; for she stands to them in a relation which is the relative product of *two* parental relations. First there is the sexual and then this is followed by the asexual parental relation, and this is a form of the grandparental relation, although neither grandmother nor grandfather.

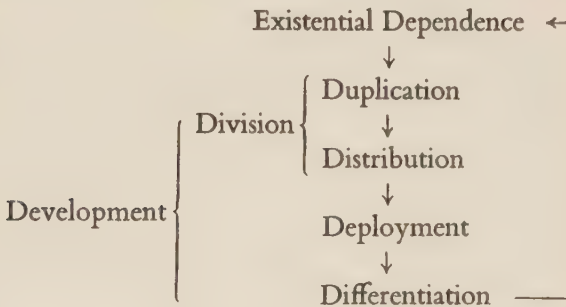
Returning to our principal theme it will be seen that there are many ways of overcoming the difficulties presented by the occurrence of existentially dependent parts in lives which are not cells. If the spatial hierarchy is not too complicated, so that division does not separate too many cells of one kind from others of another kind upon which they are existentially dependent then reproduction by division or by budding is possible. But among animals it is comparatively rare for lives that are not cells to reproduce in those ways.

Failing division or budding we have the alternative of beginning with a single cell. When a life which is not a cell begins as a cell we say that a process of *development* takes place. This is a characteristic biological process again necessitated—if new lives which are not cells are to appear—by the occurrence of existentially dependent parts in such lives. It consists essentially of the following processes: first *division* repeated until many cells have been formed. This is followed by *deployment* of these cells into masses arranged in anticipation of the establishment of the ground plan characteristic of the type of life with which we are dealing. This primary deployment may be followed by subsidiary processes of similar kind establishing parts of parts, and parts of parts of parts, in the manner of a spatial hierarchy. Finally a process of *differentiation* takes place among the cells, resulting in the production (in the appropriate relations) of secretory cells, muscle cells, nerve cells, covering cells, blood cells, etc. When this is complete division among many D-lines will have ceased and the resulting terminal cells will form the adult body, except in places where there is loss of cells, such as in the skin or among blood-cells. There a stock of link cells must remain from which losses can be made good. Under pathological conditions cells which would normally be terminals may become link cells and result in the formation of cancers and new growths. The

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study of the conditions which normally lead to the inhibition of division is thus of great importance.

The occurrence of differentiation among the cells of a **D**-hierarchy means that a cell-process distinct from duplication and the distribution of duplicates among new cells takes place. There is a process which we can call *elaboration* yielding cell-parts which can be called *elaborates*—parts which are *not* duplicates of parts occurring in cells standing in some power of **D** to the cell in which elaboration is taking place, but appear now for the first time in the life concerned. The production of different kinds of elaborates in separate cells—muscle-cell elaborates in some, nerve-cell elaborates in others, and so on, leads to differentiation and the establishment of mutually existentially dependent groups of cells. An aggregation of cells all of the same kind does not lead to a spatial hierarchy of a higher level than the cell because there is no existential dependence among the cells. This is well illustrated by the filamentous algae which are simply threads of similar cells. Existential dependence thus results in a kind of cycle of processes:



Before concluding a brief reference must be made to two kinds of parts of lives which are not cells which have not yet been mentioned. These may be called *secretions* and *accretions*. Secretions are parts which begin as parts of cells and end outside cells. These are exemplified by tears, drops of digestive secretions and the hard parts of bones. In most cases the shells of animals are formed in this way. But cases are known where shells are formed from materials picked up from the environment. Such parts would exemplify what is here meant by accretions. The clothing, the houses and the tools of human beings are also accretions; they are parts which have not begun inside the cells of the animal which uses them. Machines are also accretions; they are also existentially dependent upon the persons who make them.

J. H. WOODGER

Perhaps the persons who make them are also becoming existentially dependent upon them.

It was pointed out above how the branch of biology called physiology was concerned with existential dependence. Two other characteristic branches of biology are morphology and genetics. Morphology is concerned with the comparative study of spatial hierarchies from the structural point of view. Genetics owes its existence to the existence of the parental relation.

In conclusion we may say that biology is concerned with objects whose parts (or some of them) exhibit hierarchical order in space and are themselves ordered by hierarchies whose fields are extended in time; this type of order being connected with the occurrence in these biological objects of parts which are existentially dependent upon one another.

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PHYSICS AND BIOLOGY *

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I

TODAY the task is more imperative than ever before to investigate the so-called living sciences and to find out in what respects they resemble or differ from physics. Most of us are in agreement nowadays that we cannot expect, say, biology, to have theories and laws of the same type as physics. We have learned to respect the fact that the model of particles in motion is not adequate for describing living processes. Biologists no longer want to imitate Newtonian physics when they theorise. There remains, however, the suspicion sometimes—perhaps more among philosophers than among biologists—that at least the logical and semantic structure of a biological theory should be the same as that of a physical theory, however much the theories differ in subject-matter, or in the objects they treat of. We have given up the ideal of mechanicism—I do not want to call it ‘physicalism’ since this has other connotations—but we may still harbour the ideal of a methodological mechanicism. We may still believe, that is, that biological theory should be expressed in terms of a differential equation of the second order like mechanics, though of course the interpretation of the two theories would differ from one another. I think that, while admitting that there is something in this wish, it may seriously mislead us all the same. Logic and mathematics are formal, in the sense that they fit any subject-matter: but what kind of formalism may be used depends on the subject-matter.

Few people nowadays would impose upon biology, for example, the causal pattern as we know it from physics. This is not merely because they have accepted the fact that, in spite of its great success, physics is not the basic theory to which all others can be reduced: physics, biology, psychology, etc., treat of fundamentally different objects. It is also because the causal structure in physics has become

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less simple and less rigid—statistics has loosened the hold which necessity seemed to possess over the realm of inanimate things. Determinism as we know it from the interpretation of Newtonian mechanics current in the last century is definitely out. With it must go the prejudice that the mathematical structure which was believed to represent this determinism, i.e. the differential equation of the second order, has to be employed in biology. From such an equation, given the initial and boundary conditions, a unique solution can be derived; and this solution is then interpreted to describe the unique and necessary behaviour, i.e. an orbit, of a mechanical particle. If we remember that biological and psychological processes are ‘over-determined’, that there is ‘multiple causation’, then we realise that the search for a differential equation may be very inappropriate in biology. And so on.

I do not wish to talk here about laws in physics and in a ‘living’ science—it would be too large a subject. I must and want to confine myself to the concepts which Professor Woodger has chosen for his topic, illustrating the structure of biological theory. This is the whole-part relation and what follows from it, namely, physiology, morphology, and genetics, as constituents of biology. This then is a problem of methodology, of what kind of theory to construct, of what type of logical structures to incorporate into a theory. Can we have a physiology, morphology, or genetics within physics?

Let me say in anticipation of a more detailed argument that, I think, a morphology and genetics, certainly, and perhaps even something like a physiology can be found in physical theory as well. However, we must be reasonable in making the comparison. Usually we compare a biological theory with Newtonian mechanics or even with quantum mechanics, that is, with a highly developed, abstract theory. Naturally we find few if any points of comparison: more often than not we are at a loss to say anything, for we need some common ground which we are hard put to find under these circumstances. My idea is to compare biological theory with a physical theory that is relatively undeveloped and new and therefore simple in structure: this is the theory of radio-activity and, more importantly, the new and quite tentative theory of *strangeness* describing the behaviour of elementary particles.

The main fact is, in my view, that biological objects are time-dependent and that biological processes are genuinely dynamical, that is,

result in irreversible changes. This is in contrast to the usual physical object, e.g. the Newtonian particle, which is permanent and only suffers space-time displacement. Even if more complicated physical processes are considered, there are always one or more conservation principles. Classical thermodynamics is only *thermostatics*, correctly speaking; for the three basic ideas of closed system, equilibrium state, and reversible path preclude an over-all genuine change of the system (though they allow a partial change as measured by the entropy).

The relation of existential dependence, according to Professor Woodger, is the basic principle of biology. Living things are wholes containing parts that stand in this relation to one another. Thus the whole/part relation is the basic concept. Parts that belong to the same level need each other for continued existence, and this is physiology. The parts also exhibit a spatial hierarchical order, or levels; and thus a morphology comes about. Finally, the wholes are 'time-extended': there is the parental relation between wholes, development occurs; and we have genetics.

If we look at radio-active atoms or unstable, elementary particles, we can *almost* identify them with a biological cell. They satisfy three out of the four conditions Professor Woodger has laid down for cells. (i) Every particle is time-extended and has a beginning and an end in time; (ii) the beginning of every particle is either part of the end of a previously existing particle or is formed by the union of the end of two previously existing particles; (iii) every particle has existentially dependent parts. The fourth condition, that every cell has an environment, does not find an analogue in radio-activity at least, since decay of particles is totally independent of external influence and an intrinsic characteristic of the atom or particle.

There are four radio-active series or families. A parent atom or nuclide produces a daughter-nuclide by means of three possible activities, i.e. α -, β -, γ -decay. This gives rise to a hierarchy, starting from the original radio-active atom and ending in the stable end-product. An illustration is given by the thorium radio-active series.

Physicists were of course always aware that they had taken over their terminology from the biologists. There is also a historical, and often forgotten, connection between cell and atom. For both originally stem from the Greek atom, that is, the basic building-block that is permanent and indestructible out of which everything else in nature is constructed. Moreover, all change is explained by the shifting combination of these building-blocks, so that there never is any genuine

destruction or loss. This concept was the outcome of the Ionian school of *physiologi* from which modern physics, and science as a whole, developed. The cell is the analogue of the atom in the living sciences. And just as the atom, in modern times, showed signs of splitting, became divisible, so did the cell. The split is always in the direction of smaller, but indestructible and therefore final, building-blocks. In physics, we ended up with the electron, proton, and neutron as the only stable, elementary particles; and we had to admit a host of unstable particles, mesons, as well as positrons and neutrinos. In biology, it is, I suppose, the gene that is taken as stable and immutable constituent. Therefore, it is no surprise that such far-going parallelism can be found between radio-active families and cell development.

Indeed, corresponding to the biological evolutions we have the evolution of the earth, or the solar system, or the galaxy, or even of the whole universe that is calculated upon the radio-active decay of elements. Thus the evolutionary aspect and genetics are very similar in physics and biology. There is only this difference: the life-times involved in physics—the half-lives of radio-active or unstable particles—range from microseconds to millions of years. In biology the range is very much smaller.

There is also a spatial hierarchy—a kind of morphology among the atoms. For neutrons and protons make up a nucleus, nucleons and electrons form an atom, atoms combine to molecules, molecules form compounds, or crystals, etc. This relation we see—at least to some extent—in spatial terms. There is only this difference: the spatial structures in physics are very much simpler than those of biology.¹

Finally, we come to what may be the analogue of physiology in physics. The relation of 'needing' is too anthropomorphic, too antagonistic to the inanimate realm, for us to want to use it. All the same, the combination of atoms into molecules, for instance—that is, the phenomenon of chemical valency—has been sometimes regarded from this viewpoint. Indeed, our terminology in physics has in fact been derived from biology. The alchemists did say that the force of valency represents the need of one atom for the other. We speak today of affinity, e.g. the alkali atom and the halogen atom have a strong affinity for each other—electron affinity—and form alkali halides. It is true, however, that one or the other atom can happily survive without

¹ It has been suggested to me that the stars furnish a better example of morphology, i.e. stars, clusters, galaxies, etc., since it is achieved through a classification of more directly observed objects.

their need being satisfied—unlike the biological case. There is then this difference: chemical affinity can be described in simple terms, as a matter of energy balance and electron exchange which are subject to conservation principles.

3

Professor Woodger remarks, at the end of (the first version of) his paper that biology exhibits three main, conceptual difficulties as compared to physics. First, generalisations are almost impossible to make; second, there is little mathematical formalism to bring about a 'scientific' language; third, biologists have not yet learned to formulate their laws as functional relations.

Similar difficulties are encountered, however, if we look at the most recent theory of elementary particles in physics. This so-called strangeness theory is unlike any theory that has ever been formulated in physics; and it is much more concerned with classification and direct observation as biology is often said to be. It is quite impossible, at least at this moment, to imagine any other, more conventional, type of theory for describing the creation and destruction of particles: we seem to have struck rock bottom. Thus it may be of interest to the methodologist to describe this theory briefly.

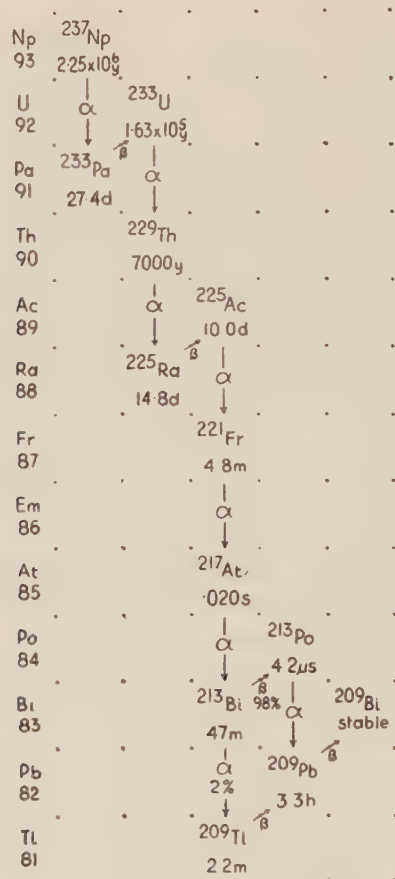
The great, and quite inexplicable, variety of elementary particles is arranged according to four classes: the heavy baryons, the intermediate mesons, the light leptons, and the photon.

Similarly, the interaction between particles appear to be in four classes. The strongest interaction, among baryons and mesons, is due to nuclear forces; the much weaker, electromagnetic interaction connects all charged particles (and those with electric or magnetic moments) through the mediation of photons; a still weaker interaction leads to the β -decay (of μ , n , K) and the μ -decay (of π - and K -mesons) which involve the neutrino; finally, an interaction of about the same strength as the β -decay leads to the decay of the strange particles.

The relative strengths of these interactions is estimated by the lifetimes of the excited states whose decay they govern. To cut the story short: certain anomalies are found, namely, that the decay of some of the particles is unexpectedly slow—instead of an expected mean life of 10^{-23} sec., the observed value is 10^{-9} sec. The basic assumption is made that these are 'strange' particles that cannot be produced in a

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collision except in association with other such particles; and that each particle carries away some quantised quantity which the other needs in order to decay. In a nuclear collision this quantity is conserved in its total amount, but its conservation is not rigorous and, indeed, must be violated in the decay process. This quantity is the 'strangeness'



The neptunium (4nH) radioactive series.

(it is a 'generalisation' of the isotopic spin which, in turn, is a 'generalisation' of the ordinary spin . . .). If Q is the charge, A the mass number of the particle, T its isotopic spin quantum number, T_s the component of T , and S the strangeness, then: $Q = T_s + \frac{1}{2}A + \frac{1}{2}S$. By choosing T , A , and S appropriately, we can describe the kind and number of particles that may occur in a nuclear collision.

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The main methodological problem here is then, as in biology: How is such a simple theory tested? The theory does not predict anything; it does not predict that the result of a collision between two particles is such and such, because there is no way of deliberately bringing about such a collision. The natural decay of unstable particles is not under our control. All we can say is that, having observed a certain, naturally occurring collision, the resultant particles fall into the scheme provided by the theory. Similarly, within radio-activity, we cannot predict *a priori* whether a certain, artificially produced (transuranic) element, for instance, decays into this or that element. We can only find out that, if the decay is of such and such a kind, then the resulting element will be this or that. Or, to take still another example, we do not know *a priori* how to fill the gaps in the Periodic Table (they have, in fact, been filled by now) or to predict which, or how many different, transuranic elements can be produced. All we can say is that, whatever element we find, will fit into the scheme the theory has provided.

I think this situation is interesting, however simple it may be; there are similarities with biology. The standard model for testing a hypothesis, namely, to predict the next occurrence of a known event in space-time, e.g. an eclipse, or a particle in its path, does not apply here. We must, however, distinguish between the actual experimental arrangement and the interpretation of the results. We may, for example, establish the existence of a new transuranic element by means of a mass spectrograph, that is, by observing a beam of particles, or a deposit of particles, in a certain space-time region. That this element exists, and that it does fit into the scheme, that it possesses the properties the Periodic Table predicts, is judged by the consistency of our interpretation of the actual results. The space-time experimental arrangement is merely a convenience: it is not an essential part of the theory. In this particular instance, we have been able to translate or, better, interpret a classificatory theory in terms of the space-time theory underlying the experiment. This is not, nor need it always be, the case. The numerical calculation, the mathematics, belongs to the theory of the experiment, not to the theory to be tested.

4

I want to sum up the argument. When we compare physics and biology, then we must take care to choose appropriate theories,

that is, theories of the same level of 'abstraction'. In physics as well, there exist simple theories that are classificatory rather than causal (in the ordinary sense of this term). Such theories are concerned, as are the corresponding ones in biology, with genuine processes, creation and destruction. The theories are not very mathematical and seem intrinsically unsuitable for an advanced formal treatment. The theories are not aiming at predicting an event in space-time; they are tested by their coherence or integrative power. Morphology and genetics, instead, replace, so to speak, the space and time relations of the entity described by the theory.

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CHAIRMAN'S COMMENTS ON THE CONTRIBUTIONS BY WOODGER AND HUTTEN *

C. F. A. PANTIN

PROFESSOR WOODGER has drawn attention to the importance of the hierarchical organisation of existentially dependent parts as a characteristic of living organisms. The question then arises, how far is such organisation also to be found in 'physical objects'? Dr Hutten suggests that in part an hierarchical relationship can be seen in a natural transmutation-series of radio-active atoms, though, unlike the living cell, there is in this case no influence of the environment on the process of division.

Some 'physical' systems in fact show an even more complete resemblance to living ones. A drop of ink allowed to fall into a glass of still water sets up a vortex ring. As the ring descends it expands and generates a number of subsidiary rings. These descend and expand like the parent ring; and in due course they may produce a third generation of vortex rings, and so on.

Just as in the case of a living cell, we can say here (1) every vortex ring is time-extended and has a beginning and an end in time; (2) the beginning of every vortex ring (except of course the first) is part of a previously existing vortex ring; (3) every vortex-ring has existentially dependent parts; (4) every vortex-ring has an environment: in this it resembles a cell more closely than does Dr Hutten's transmuting atom.

It may be asserted that such hierarchical systems are not characteristic of 'the usual physical objects'. But reflection about complex physical systems suggests that by 'the usual' is meant 'those usually investigated by physicists'. The tendency to select the simplest systems for scientific analysis can lead to a systematic error in our description of the variety of phenomena unless we are constantly alive to what we have done.

Nevertheless, this hierarchical relationship is such a striking feature of living systems that it is of interest to consider whether there are any

* Given at the Fourth Annual Conference in Philosophy of Science, Cambridge, 25-27 September 1959.

other features of the relationship which are particularly characteristic of living things. Two occur to me:

(1) In living things, what seem to us important features recur at different levels of organisation. Thus, individuality recurs at several distinct levels; that of the cell, that of the multicellular organism, that of certain colonial organisms (particularly in certain relatives of the corals). In the opposite direction, it can be seen in certain parts of cells, such as mitochondria, which like the cell itself exhibit a hierarchy-generating relationship.

A striking example is seen in certain slime-moulds. In species of the genus *Dictyostelium* the organisms are small amoebae. In the feeding-phase large numbers of these wander about exhibiting amoeboid movement; the pushing out of protoplasmic processes for locomotion and for the capture of food. Under certain conditions these amoebae aggregate together to form a compact sausage-like mass. In this genus the amoebae retain their individuality; they become, as it were, 'cells' of the large mass. But the large mass itself as a whole then begins to move as a giant 'cellular' amoeba. Moreover it exhibits directed behaviour comparable to that seen in 'unicellular' amoebae. Thus we have amoeboid movement and behaviour both at a unicellular (or non-cellular) level and at a multicellular level.

This recurrence of significant characters is of some importance in scientific analysis. Much work is being done on the neurological basis of behaviour; of sensation, of learning and memory, of the directed activities and power to choose. But it is important to remember how many of these features are also evident at the non-cellular level, as in the 'purposive' behaviour of amoebae. It is clear therefore that these features are not solely the concomitants of the nervous organisation, such as we see in multicellular animals. Any general description of the class of mechanism to which a 'learning machine' must belong must therefore be independent of the condition of cellular organisation; though it must be expressible in terms of nerve-cell organisation as a special sub-class.

It might well be that we could further restrict the specification of a 'learning machine' till its requirements could only be met by a machine composed of nerve-cells. But these additional requirements would not be the essential characters of a learning machine.

The properties of a system of synaptic junctions between nerve cells may provide the basis for a learning machine, but we cannot say that the essential feature of a learning machine is such a system of

synaptic junctions. The practical importance of this is considerable because we so often find in the physiological analysis of function that a functional need is met not by the development of a single system, but by the simultaneous presence of several systems working on different principles but all meeting the same need. I do not wish now to discuss this interesting matter further: I would only point out that unless its importance is appreciated a biologist may think his analytical task ended when he lights upon the first system in the organism which meets the physiological need he has specified.

(2) The second feature of living things which I wish to discuss is the possession of existentially dependent parts. Dr Hutten suggests that we can see these also in atomic organisation. But there seems to me to be an important difference in the two cases. Characteristically for living organisms we can ask 'What is this part *for*?' and we expect a useful answer. We cannot usefully ask that about the parts of an atom. We can of course ask it about the parts of a machine. If we find a carburettor on a scrap heap we can usefully ask what it is for. From such a part in isolation much can be predicted about the machine of which it is a part. The same is true of the parts of animals: spectacular predictions of this sort have been made from a part of a single bone.

This well known quality of the parts of living organisms is important in the analysis of biological systems. A large class of biological investigations begins with inquiry about the functional purpose of some part. That inquiry is based on anatomical study. The answer to the inquiry is made through a physiological investigation of the properties of the part. When this is made we not only learn something of how the part functions but also see further anatomical implications in the manner of action of the part. Thus, examination of a limb may suggest that it moves in a certain way. Physiological demonstration that it does so may reveal that its manner of action seems to imply the presence of proprioceptive sense organs in certain places in the limb to give information about its position and movement to the central nervous system. The result of the physiological analysis is thus a hypothetical anatomical model. We then return to the anatomical study of the limb to verify this model. That more minute investigation leads to further more detailed specification of function—which is in turn tested physiologically; and so on.

This procedure is possible because the parts of the organism can themselves be divided into existentially dependent parts. Clearly

there must be a limit to this, when we subdivide the part down to the molecules, or even atoms of which it is composed.

An interesting illustration of this can be seen in some parasitic organisms. Certain parasitic Crustacea (*Sacculina*) have a larval stage consisting of a sac-like body about $1/16$ th mm. long containing undifferentiated reproductive tissue. The larva attaches itself to the host (a crab) and this tissue is forced through a special hollow process into the body of the host, to form a growing mass of cells which feed on the tissue of the host. We are not concerned with the rest of the life history. The parts themselves bear a functional relationship to each other and are characteristic of the organism which produces them.

A remarkable parallel organisation to this is to be found at a molecular level of organisation; that of the bacteriophage, a virus parasitic on bacteria. The organism in this case is a sac about $1/10,000$ mm. long containing reproductive substance, deoxyribonucleic acid, which is forced through a tube which is attached by specialised threads to the surface of the bacterium. The nucleic acid is forced through the tube into the body of the bacterium, where it multiplies at the expense of the bacterial substance. As machines the two parasites are remarkably similar in spite of the enormous difference in scale of organisation.

But in the phage the parts show evidence of individual molecules of which they are composed. Whereas an isolated part of a large organism can give us unique information about the functional structure from which it is derived, the molecular functional parts can only give us ambiguous information. An isolated molecule of collagen might have been derived from any one of a variety of different structures or organisms.

Nevertheless, these large molecules are in some respects intermediate between unique large functional parts and atoms which tell us nothing of the structure from whence they were derived. Thus if we find molecules of a haemoglobin we can infer with moderate probability that it was the respiratory pigment of a vertebrate animal. We may be wrong, for it may have been derived from other sources, where it had other functions. Thus it might have come from the roots of leguminous plants—where it is concerned with nitrogen fixation. But if all we know about the substance is that it is haemoglobin it is still moderately safe to wager that it was the respiratory pigment of a vertebrate animal.

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THE DOPPLER EFFECT AND THE FOUNDATIONS OF PHYSICS (II) *

HERBERT DINGLE

10 *An Unsolved Problem*

AT present the position is, to put it bluntly, that our conceptions are inadequate to explain the Doppler effect. Whatever theory we adopt, we can imagine situations in which we cannot predict what would be observed. Here is an example.

Consider two exactly similar observers, A and Z, alone in space and relatively at rest at a distance X apart. They carry synchronised clocks, and continuously radiate the same monochromatic light. At the same instant, $t = 0$, they fire identical rockets in the direction $Z \rightarrow A$ with equal momenta. Will either of them observe a Doppler effect, and, if so, when?

If, according to its definition, a Doppler effect is necessarily associated with relative motion between the bodies concerned, they will not, for at no time is there any relative motion between A and Z. But if there were a third observer, A', originally at rest beside A, who did not fire a rocket, we have no reason to doubt that they would both observe A's light displaced from the moment of firing the rockets. If, then, Z does not observe A's light displaced, it must be that the effect of A's motion on A's light must be transmitted instantaneously to Z, although the observation of A's rocket-firing operation would not be possible to Z until time X/c later. This is difficult to believe in the light of present conceptions, and, in fact, it is often assumed without question ¹ that the Doppler displacement would take the same time to reach a distant point as the light itself. Suppose, then, that it does take that time. Then during the interval from $t = 0$ to $t = X/c$, Z, because of his own motion, observes A's light displaced by a definite

* Continued from the previous Number.

¹ See, e.g. J. H. Fremlin, *Nature*, 1957, **180**, 499, and C. G. Darwin, *Nature*, 1957, **180**, 976

amount, $d\lambda$, from which he can calculate a velocity dV . What is that velocity? There are only two bodies concerned, A and Z, and they are never in relative motion (A' is imaginary, and the rockets cannot determine dV , for their velocity is quite indefinite; only their momentum determines the subsequent motion of the bodies from which they are fired). dV must measure a velocity with respect to no physical standard, and the possibility of such measurement is contrary to the postulate of relativity.

Still more surprising results follow if we adopt this supposition. Suppose that, at $t = 0$, A alone fires a rocket. Then, up to the time $t = X/c$, Z will go on observing A's light at its earlier frequency. If, then, at that instant he fires his own rocket, he will again be reduced to rest with respect to A, and since he perceives the effect of his own change of motion instantaneously, it will cancel the effect of A's, which otherwise would just have become visible, and all spectrum observations will be to him as though there had never been any relative motion at all between him and A. But A would, in fact, have drawn nearer to him by a distance $dV \frac{X}{c}$, and if, at any later time, he measures the

distance of A, he will discover this. If he trusts to the Doppler effect to tell him of his motion relative to A, he will then have to conclude that the only effect of his attempt to recede from A has been to bring A nearer without causing any relative motion at all.

The process could be continued. At the instant $t = \frac{X}{c}$, A also could fire another rocket. Its effect would not be observed by Z until after a further time, $\frac{X}{c}(1 - dV/c)$, and by then A would have drawn still nearer by an amount $\frac{dV \cdot X}{c}(1 - dV/c)$. By a succession of such acts, A could approach to contact with Z, without Z observing any Doppler effect at all, while A would observe one throughout. It would be a sort of Achilles-tortoise race. Z, now revealed as Zeno the Eleatic, could sneer at all the attempts to fault his argument, and triumphantly point to the absence of a Doppler effect as proof that the alleged motion of A was indeed an illusion.

We appear, then, to have to choose between two impossibilities— instantaneous transmission of a phenomenon and observable motion

with respect to a non-existent standard.¹ I think that very few, after reflection, would venture to predict with confidence what would actually be observed in such a case: our experimental knowledge is too slight, and our theoretical ideas too imperfect, to permit anything more than a vague feeling of probability. There is, however, one way of escape from the dilemma, and that is to suppose that light itself can act as a standard with respect to which velocities are measurable. This would enable us to allow a delay in transmission of the Doppler effect without invoking a non-existent standard for velocity measurement: our dV would be defined by the fact that the light from A, received by Z at the beginning of his motion, would move with velocity $c - dV$ with respect to Z. But this means adopting the ballistic theory insofar as the velocity at emission of the light is concerned, and rejecting Einstein's second postulate, that light moves with the same velocity with respect to relatively moving bodies. The only way to save both of Einstein's postulates is to suppose instantaneous transmission of the Doppler effect: otherwise one of them is bound to go.

II *Faraday's Conception of Ray Vibrations*

Instantaneous transmission of anything at all is usually regarded as incompatible with Einstein's theory, though I am not aware that it necessarily violates the basic postulates of the theory, and there is certainly nothing in the ballistic theory to prohibit it. It is, however, inconsistent, so far as electromagnetic phenomena are concerned, with the Maxwell-Lorentz theory, which Einstein's theory was designed to justify. If we allow it to be possible, we are led to a very suggestive line of thought.

The story is well known how Wheatstone, when about to deliver a Friday evening Discourse at the Royal Institution in 1846, was suddenly seized with stage-fright, and at the last moment ran away, leaving Faraday with the duty of entertaining an expectant audience at a moment's notice. It is on that account that, ever since, an assistant

¹ In a letter to *Nature* (1957, 180, 1275) I described a third result, which involved equal finite delays at both ends in observing a change of relative motion, however caused. This would satisfy the postulate of relativity, but the argument leading to the amount of the delay presupposed that the spectrum frequency was identical with the frequency of reception of light elements. This we have now seen to be unjustified. There is therefore now no ground for accepting that result, and the only delay compatible with relativity is zero.

has stood unobtrusively between the stairs and the lecturer when he is about to enter the auditorium. What is not so well known is what Faraday said. He did the only thing possible: he gave expression to ideas that were at the moment passing through his mind, which he called 'thoughts on ray vibrations'. He presented them with much diffidence, and very tentatively. As he wrote afterwards, 'I do not think I should have allowed these notions to have escaped from me, had I not been led unawares, and without previous consideration, by the circumstances of the Evening on which I had to appear suddenly and occupy the place of another.' These notions are preserved for us in a letter which he wrote to Richard Phillips, which was published in the *Philosophical Magazine*.¹ It must be remembered that at that time the luminiferous ether was held to be as unquestionable as Einstein's theory is now, and publication of such heterodoxy as Faraday's was possible only in a journal which then merited its title and whose function has now to be performed by such journals as this.

In Faraday's view, the 'ultimate atoms' of matter are centres of force only, and do not have 'a definite form and a certain limited size. . . . That which represents size may be considered as extending to any distance to which the lines of force of the particle extend: the particle indeed is supposed to exist only by these forces, and where they are, it is.' Light and such vibrations 'occur in the lines of force which connect particles, and consequently masses of matter, together. . . . I do not perceive in any part of space, whether (to use the common phrase) vacant or filled with matter, anything but forces and the lines in which they are exerted. . . . The view which I am so bold as to put forth considers, therefore, radiation as a high species of vibration in the lines of force which are known to connect particles and also masses of matter together. It endeavours to dismiss the aether, but not the vibrations. . . . The aether is assumed as pervading all bodies as well as space: in the view now set forth, it is the forces of the atomic centres which pervade (and make) all bodies, and also penetrate all space.'

This suggestion met with the inaudible response usually accorded to what is unfashionable, and the echoes have reverberated with the same amplitude ever since. Faraday's extraordinary instinct took him beyond what it was possible to apply at that time, and when, many years later, a formal electromagnetic theory of light did appear, it was built

¹ *Phil. Mag.*, 1846, 28, 345

on the conception of a fixed universal ether, which has had slowly and painfully to be abandoned, leaving a system of equations which indeed fit observations so far as they can be tested, but lack support in conceivable physical foundations. But at the viewpoint from which we can now survey the Doppler effect, the 'ray vibrations' take on a new significance. Suppose that each atom (we need not make thought more difficult by refusing to *imagine* the atom apart from its actually inseparable entourage) is the origin of 'lines of force' (let us call them merely 'rays', now that 'force' has a more specific meaning than in Faraday's time) proceeding outwards in all directions. No relative motion is possible between an atom and its rays: if we regard the atom as moving, its rays move instantaneously with it—which, of course, must necessarily follow if, in accordance with the postulate of relativity, we are always entitled to choose a coordinate system in which the atom is at rest. A beam of light 'proceeding from the atom' consists of a wave travelling along a ray with an invariable velocity c —invariable, that is, with respect to the atom and to the ray; its velocity with respect to a receiving body would be, on the ballistic theory, the resultant of c and the relative velocity of receiver and emitter, and on Einstein's theory, c (this is unpicturable, of course, whatever we may assume).

On this view, transmission of the Doppler effect would necessarily be instantaneous. Since the ray is, in effect, a part of the emitting atom, it moves with respect to the receiver at the instant at which the emitter moves, and the light waves which it carries, having always the velocity c with respect to the ray, have the velocity $c - dV$ or $(c - dV)/(1 - cdV/c^2)$ (according to the theory we adopt) with respect to the receiver, exactly as though it were the receiver, and not the emitter, which had moved at that instant. But, of course, a Doppler effect would not be perceived unless the ray carried waves, at the position of the receiver, that had been emitted some time earlier. If a new star flares up, we perceive a Doppler effect, corresponding to the expansion of its atmosphere, simultaneously with a sudden accession of brightness. Both these events must be assigned to an instant perhaps many light years before. When the rays from the atoms in the expanding atmosphere moved at the time of the outburst, there were no light-waves on them, at the position of the Earth, by which the motion could be perceived. Such weak waves as were there were lost in the (very faint) light of the star as a whole, which did not change its motion when the outburst occurred. It is quite otherwise, of course, in the

case of a binary star, for example. There the components have long been visible, and their rays move to and fro with respect to the Earth, giving us the *present* motion of the star. That is why de Sitter's attempt to disprove Ritz's theory was irrelevant.

In terms of this conception we can form a much clearer idea of what we might expect to observe if the rocket-firing experiments could be performed. The Doppler effect being observable instantaneously at both A and Z, no shift of spectrum lines would occur at either place in the experiment in which the rockets were fired simultaneously. In the Achilles-tortoise experiment, both A and Z would observe the same effect at all stages of the process—namely, a displacement to the higher frequency side throughout. Zeno would be confounded once more. The postulate of relativity would be completely satisfied in both experiments: where there is no relative motion there would be no spectrum displacement, and where there is relative motion the spectrum displacement would yield the corresponding velocity at all times and would be perceived by both observers alike.

Of course, it is not necessary to suppose that the rays 'exist' in the sense that they admit of independent detection by observational means or that it would be meaningful to ascribe to them physical properties such as tensile strength and so on. The philosophy of science of the present day is inconsistent with the naive realism that compelled nineteenth-century physicists to add the metaphysical endowment of 'existence' to the qualities that alone are necessary to justify the introduction of a concept into physics. The rays are kinematic entities only, with no more 'substance' than space or time—unless, indeed, further experience makes it appropriate to amplify their definition. We may, with Faraday, regard them as the only entities in the universe, displacing the conception of point-atoms by that of indefinitely extended 'atoms', and substituting for the axiom that no two bodies can occupy the same space the axiom that all bodies occupy all space; or we may hold both conceptions together, regarding atoms as entities conforming to the first axiom and rays as entities conforming to the second—'atom' being merely a synonym for the origin of electromagnetic waves. This is entirely a matter of convenience, and at the moment the latter view seems to hold the advantage. If, for instance, we have to distinguish between the frequency of reception of light and the spectrum frequency, in the manner suggested earlier, our problem seems better expressed as that of determining how a wave on a ray from one atom interacts with another

atom when they meet, than as that of distinguishing between the initial and terminal points of a light-wave in a homogeneous system of rays. But no matter of fundamental principle is involved.

12 *Summing-up of the Situation*

It is not the purpose of this paper to develop the consequences of this possibility, which may well be very great. In cosmology, for instance, the motions observed in the distant nebulae—if indeed the spectrum displacements do correspond to motions—would be those existing at the present time and not at earlier epochs in the history of the universe. But the point of philosophical interest is what we can learn from the Doppler effect concerning the fundamental nature of motion, and the conclusions we have reached may be summed up in the following way.

Accepting the Doppler effect as an experimental fact, two alternative theories are available for its explanation—Einstein's special theory of relativity, and the ballistic theory of Ritz in a somewhat amplified form. Einstein's theory is based on two postulates—the postulate of relativity, which says that motion is essentially a relation between two or more bodies and is a meaningless term when applied to a single body; and the postulate of constant light velocity, which says that all measures of the velocity of light with respect to a body, whatever the motion of that body with respect to anything at all, will yield the constant value c . The ballistic theory also may be based on two postulates—the postulate of relativity, and a different postulate of constant light velocity, which says that the velocity of light is always c with respect to the body (in the last resort, the atom) that emits the light, no matter how that body may be moving at the time of emission or later.

Einstein's theory of necessity makes a complete divorce between the velocity of light and its frequency, because the velocity is independent of the motion of source or receiver and the frequency varies systematically with the relative motion of those bodies. It yields a formula for the Doppler effect agreeing with observation, within the limits of experimental error, on the added assumption that light consists of electromagnetic waves obeying the Maxwell-Lorentz equations, but unless it assumes that a change of relative motion between two bodies that are receiving light from one another is made *immediately* evident to both of them by a Doppler effect, one of its postulates must

be violated. If it does make that assumption, the Maxwell-Lorentz theory is violated. But without the Maxwell-Lorentz or some alternative theory, a Doppler formula cannot be derived. It follows that the Doppler effect provides us with a phenomenon incompatible with the requirements of Einstein's theory as it is at present understood.

The ballistic theory, on the other hand, supposes a definite connection between the velocity and the frequency with which light is received from an emitting body. The Doppler effect formula resulting from that connection, however, is only approximately identical with that yielded by Einstein's theory, which appears to accord with experiment, but agreement is reached if a simple relation, not that of identity, and with no theoretical basis yet known, is assumed between the frequency of reception of light and the quantity having the dimensions of frequency which is derived from spectrum observations. Like Einstein's theory, the ballistic theory has its first postulate violated by a delay in transmission of the Doppler effect, unless light can act as a reference body for the estimation of velocities; but, unlike Einstein's theory, it could allow light so to act by the deletion of the words 'or later' in its second postulate. This, however, would imply that the source could change its velocity with respect to the light between emission and reception, and, further, it would imply a standard of rest entitling one to ascribe that change to the source, which is contrary to the first postulate. Furthermore, if the Earth be chosen as such a standard, and the velocity of light be assumed constant with respect to it, the binary star phenomena cited by de Sitter prove that the modified postulate is not in agreement with fact. Hence immediate transmission of the Doppler effect is necessary on this theory also. This is provided for by Faraday's conception of 'ray vibrations'. With this supplement the ballistic theory gives a satisfactory account of the Doppler effect, but at the expense of the Maxwell-Lorentz electromagnetic theory.

13 *General Comparison of the Theories*

Einstein's special theory of relativity and the electromagnetic theory lie near the foundations of theoretical physics. Their rejection in face of their remarkable triumphs may well seem inadmissible, and is certainly not to be lightly suggested. There is, however, other evidence pointing in the same direction. I have shown elsewhere ¹

¹ *Bull. Inst. Phys.*, loc. cit.

that the experimental results which have hitherto been held to confirm Einstein's special relativity theory are equally consistent with what I have here called the ballistic theory, if the velocities which in those experiments have been represented by V (and, in fact, have been calculated from the electromagnetic field equations) are really $V/\sqrt{1 - V^2/c^2}$. This would entail, of course, a re-interpretation of the field equations: this will be discussed in more detail presently.

The ballistic theory of the present paper is entirely conformable to this change. Its application to the Doppler effect takes us a step towards the amendment of the electromagnetic equations by the picture which it evokes of the 'ray vibrations' envisaged by Faraday so long ago. The Maxwell-Lorentz equations were constructed on the basis of a fixed universal ether. By common consent, that basis is now regarded as illusory. The equations have been retained because of their consistent agreement with experiment for low velocities but no physical principles are known from which they can be derived: that agreement is preserved in the proposed change. It is certainly a task for the mathematical physicist now to attempt the construction of an electromagnetic theory based on the idea of ray vibrations instead of that of an ether which it is no longer possible to credit.

14 *The Nature of Motion*

The most urgent need is, of course, an experiment to determine whether, in fact, light emitted from relatively moving bodies travels at a single velocity through space. That would at once settle the issue between the two forms of the velocity of light postulate which distinguish the theories, in a manner that would carry conviction to physicists. At the same time, more general considerations belonging to the philosophy of science are relevant in order that the experimental results shall be not only accepted but understood.

The physical phenomenon with which we are concerned in kinematics is motion. As a phenomenon, motion is always a relation between two or more bodies; hence the postulate of relativity of motion is merely a truism. To speak of the motion of a single body is either to imply the existence of another which is not mentioned, or else to use the word 'motion' to represent something of which we have no experience. Until recent years it was not uncommon to speak in physics of the motion of a single body, and the tacit assumption was that of a universal 'stationary' ether. The 'relativity' that has

come into prominence during this century is simply the denial that such an ether exists and the assertion that reference to another *piece of matter* is necessary to give meaning to the statement that a single body is moving: it is not a re-definition of motion in relative instead of absolute terms.

The basic problem of physics is to describe the observed motions of bodies in terms that conform to this essential quality of relativity. To do this we define two concepts—space and time—prescribing processes which we regard as measuring certain intervals in them, and measure motion by *velocity*, which we define as the amount by which the position of a body in space changes in a given time interval. Space is conceived as entirely featureless, so that a single body can be said to remain always at the same place, or to be constantly changing its place in any way imaginable, without making the slightest difference to anything observable. This is sufficient to ensure the relativity of motion, no matter whether the *time* between two events in the history of the body is conceived as determinable absolutely or not. If we cannot assign a particular position to a body, then necessarily we cannot assign a particular *change* of position (i.e. a particular motion) to it. We are thus free to define the times of events at different places in any way we please, provided that the definition conforms to our own direct experience of time, without the corresponding conception of velocity violating in the least the essential relativity of motion.

The realisation of this fact was Einstein's great contribution to the subject, for which, whatever the fate of his theory, our debt to him is incalculable. Previously it had been taken as compulsory that time must be defined in such a way that the times of events at different places were uniquely assignable. Einstein pointed out that this was not necessary, and that we were free to define it otherwise if thereby we could attain to a simpler or more accurate description of the way in which bodies naturally move. He did in fact, define it otherwise, and the difference between his theory and the ballistic theory lies simply in the fact that (the time-measuring instrument and the zero-point having been agreed upon) in the former the time-interval between events at different places is indefinite, depending on the arbitrary way in which one regards the bodies concerned as moving, whereas in the latter it is fixed and independent of that movement.

Let us see how this works out in practice. Consider two events in the history of a single body alone in the universe. Whether these events occur at the same place or at different places is entirely a matter

of arbitrary choice. We can suppose the body at rest, in which case the space-interval between the events is zero, or moving with some finite velocity, in which case its magnitude will depend on the velocity that happens to be chosen. The time-interval, however, is not so arbitrary. The condition that time measurement must conform to our experience of what we call the continuous passage of time prohibits us from supposing that the time-interval is zero, for the body may be ourselves. If it is provided with a standard clock of the accepted type, that will, of course, give definite readings for the events. On the ballistic theory, the difference between these readings— Δs , say—is the unique time-interval between the events, but on Einstein's theory it is not: it is the time-interval only if we suppose that the body has not moved between the events. If we suppose that it has moved a distance Δx , then the time interval must be defined as $\Delta t = \sqrt{\Delta s^2 + \Delta x^2/c^2}$, where c is the velocity of light.

This seems an unnecessarily complicated choice in a matter in which we have complete freedom. Since space and time measurement is entirely a matter of definition, these quantities being concepts in terms of which to describe phenomena and not natural phenomena themselves, one would have expected the simplest choice to have been made. But what is simple in origin may involve great complexity in application, and Einstein's choice was made in order to preserve the simplicity of the electromagnetic equations, in which Δt is the measure of time implied. But will this choice allow a consistent description of ordinary kinematical phenomena? Einstein showed that it would, *if it be assumed that the motion of light in space is independent of the motion of the emitting body*. Now in so far as the emitting body can be regarded as moving in any way we like, this is literally obvious, for we cannot suppose that the light will change its behaviour in any way when we change our minds about the motion of the emitting body. But Einstein's assumption meant more than this. If we have two bodies in relative motion, we can assign what velocity we like to either of them, but the other must then have a different velocity: we cannot reduce them both to rest by any act of thought. Einstein's assumption is that the light from two such bodies will, in fact, travel through space with the same velocity. The assumption of the ballistic theory, on the other hand, is that the velocity of light with respect to its source is always the same, and therefore that the light from the two relatively moving bodies will not travel through space with the same velocity.

This is not a difference of definition, but a matter for experiment to settle.

But I think that it is unnecessary to await the result of experiment: we can see that there are internal inconsistencies in Einstein's theory which, whatever result the experiment might give, make it untenable, at least in its present form.¹ This does not mean of course, that the ballistic theory is necessarily right: some third possibility, not yet envisaged, might be the truth. To see the inconsistency of Einstein's theory, consider the following case.

Two observers-cum-clocks, A and B, relatively at rest at a distance Δx apart, confirm by light-signals that the clocks are synchronised: that is to say, each one observes that if he sends a beam of light to the other and receives it back by reflection, the time which it shows the other to read at the moment of reflection is the arithmetic mean of the times by his clock of emission and return. The criterion that the observers are relatively at rest is that their positions on a space-measuring instrument, reaching from one to the other, remain constant. That it is possible for A to be synchronised with B and B also to be synchronised with A in this way is, strictly speaking, an assumption, but its truth can scarcely be doubted, it is common to both Einstein's and the ballistic theories, and I shall take it to be beyond reasonable doubt. At an instant when both clocks read 0, A and B begin to move towards one another, and eventually meet. The motion may be regarded in either of two equally valid ways, among others. It may be supposed that A remains at rest and B moves towards him, or that B remains at rest and A moves towards him. Suppose first that A remains at rest, and consider the interval between two events in B's history, simultaneous to B with the starting and ending of the motion. The distance between these events is Δx , and if Δs is the reading of B's clock on arrival at A, then the time of the journey must be $\Delta t = \sqrt{\Delta s^2 + \Delta x^2/c^2}$. To A, the time of the journey is the time between the moment when his clock read 0 and the moment when B reached him. During that time his clock remained at rest, so it records the actual time of the journey, namely, Δt . Hence, since $\Delta t > \Delta s$, A's clock must be in advance of B's clock on meeting.

¹ Inconsistencies, that is, in the sense that the theory requires incompatible observations when applied to possible physical situations. Considered as a mathematical theory alone, it shows no inconsistency. It is because attention has been focused too exclusively on the mathematics of the theory that these inconsistencies have been overlooked.

Now clearly we can repeat this argument with A and B interchanged, and in that case B's clock can be proved to be in advance of A's when they meet. These results are contradictory, and it is obvious that the only possibility, consistent with the postulate of relativity—i.e. with the postulate that we can regard either clock as remaining at rest throughout the process—is that the clocks shall agree on meeting.

This objection to Einstein's theory has, of course, not passed unnoticed. A way of escape was first indicated by Einstein himself¹ and has since been explored in more detail by other writers, the latest being Born and Biem.² The argument is this. The contradictory results are reached by assuming that complete symmetry exists with respect to the two observers. But in fact (gravitation being neglected, as it has been in the foregoing considerations) A and B, if left to themselves, would never come together. We must apply a force to one of them to make this happen, and although the *motion* of A may be equivalent to that of B, the one that suffers this force is thereby distinguished from the other, and we have no right to suppose that this difference will be without influence on the clock readings. Not that the impulse itself necessarily changes the reading of a clock. That effect, if it happens, will depend on the nature of the impulse, and is incalculable without detailed specification; it may be ignored for our purpose. But the asymmetry affects the problem in a much more subtle way.

Suppose that A is subjected to the force. Then, without introducing any further supposition, we can say that A moves, and so arrive at the conclusion that A's reading will be behind B's when they meet. But if we say that A, although experiencing a force, remains at rest, and B, which experiences none, moves towards A, then we must suppose that a gravitational field acts on the system during the short time in which the force operates, and this just counteracts the force and keeps A at rest while it impels B, which has nothing to resist it, towards A. Without this supposition it is physically impossible for B to move as the result of an impulse applied to A. But now, A and B, being at different places, are in regions of different gravitational potential, and Einstein's theory of gravitation requires that in these circumstances B will gain with respect to A during the time of operation of the field.

¹ *Naturwiss.*, 1918, 6, 697

² M. Born and W. Biem, *Proc. Kon. Ned. Akad. v. Wetensch. Amsterdam*, 1958, B61, No. 2, 110

Calculation shows that, to a first approximation, this gain more than counterbalances the loss which B experiences on account of its motion and requires that when B and A meet, B will be in advance of A by the same amount as is calculated on the supposition that A is the moving body. The result, then, is that in such an experiment as this, the clock that experiences the physical force will be behind the other when they meet, and this agrees with calculation no matter which clock we suppose to have moved.

This most ingenious explanation, however, though mathematically sound for small velocities, fails completely if we try to regard it as representing what physically takes place. Suppose a third clock, C, to be at rest beside A and synchronised with A and B before the process begins, but suppose that it does not receive the impulse applied to A. Then it will, of course, remain permanently at rest with respect to B, no matter how we regard whatever motion takes place. When the supposed gravitational field causes a rapid relative change in the readings of A and B, therefore, it must cause the same relative change in the readings of C and B, for the difference of gravitational potential is exactly the same for the two pairs. But that means that two clocks, B and C, relatively at rest, to neither of which is anything done at all, must suddenly go out of synchronisation at the moment at which a force is applied to a third clock. This is quite incredible. The fact is that the supposed gravitational field is a pure fiction, which can have no effect whatever on the clock readings; and indeed, when we make the calculations more precise it becomes clear that this explanation not only does not give exactly the right advancement of B with respect to A, but in some cases requires that B must gain large amounts—several years, in fact—over A during the application of a force which may be operative for only a short time, perhaps a week.

No other escape from the difficulty, so far as I know, has ever been proposed. We are bound to conclude, therefore, that Einstein's theory is inconsistent with the postulate of the relativity of motion.

15 *Physical Measurements and Mathematical Symbols*

It is important to understand quite clearly what this means. It does not mean that the theory is 'wrong', in the sense that it necessarily gives a false description of phenomena. It does mean that the relation between the mathematical symbols of the theory and the quantities we measure with our accepted instruments is wrongly

conceived. It has been assumed that the Δt of Einstein's theory is identical with clock records of time intervals in certain prescribed circumstances. That leads to contradictions, which can be removed if, in those circumstances, the clock is held to record Δs —i.e. $\sqrt{\Delta t^2 - \Delta x^2/c^2}$ —and not Δt . As a consequence, measured velocities are to be identified, not with the quantity $V = dx/dt$, but with $W = dx/ds$, and it is easily seen that $W = V/\sqrt{1 - V^2/c^2}$. So long as we recognise this, it is entirely a matter of physical convenience whether we express our results in terms of dt and V , or of ds and W . The measurements we make in physics are the results of freely chosen operations. We can perform whatever operations we wish, so far as nature is concerned, but the relations between their results, which we call laws of nature, are not then at our choice.¹ We naturally choose the operations whose results are found to stand in the simplest relations with one another, but we are not bound to take those results exactly as they stand and define them as physical quantities to be represented by single symbols in our equations; and, in fact, we do not always do so. We measure 'temperature' by some physical instrument, for example, but always 'correct' its readings to what we believe a different instrument would give if we were able to make it. That is quite legitimate, so long as the correction is precisely defined. In the same way we are free, if we wish, to define 'time' as the reading of a clock modified by combination with a function of the reading of a space measuring instrument, i.e. as the quantity Δt , instead of as the simple reading of the clock.

The quantity Δt was chosen because a mass of experimental evidence shows that it is what represents time in the electromagnetic equations. We have no reason—at present, at any rate—to suppose that those equations do not represent the facts of electromagnetism, and we are therefore perfectly free to retain them so long as we understand what their symbols mean in terms of our measurements. What we can now see to be Einstein's error was to suppose that this quantity, Δt , was the actual reading which a clock would record at the arrival of a body which left a point at a distance Δx at zero time. That led him to force on kinematics a most inconvenient form of expression, that necessitated a 'contraction' of rods, a 'slowing down' of clocks, and an 'increase' of mass when bodies were 'moving', notwithstanding that 'moving' was an attribute that could be applied

¹ See the author's paper, 'A Theory of Measurement', this *Journal*, 1950, I, 5.

or withdrawn by an act of will. These purely conceptual changes all vanish if the kinematical laws are expressed in terms of Δs instead of Δt , and we recover the simple Newtonian expressions for the relations between mass, time and space measurements, in which the symbols represent the indications of instruments without modification. The importance of Δs (though not its direct significance) was discovered by Minkowski a few years after Einstein's first paper was published. What he did, as we can now see, was to rediscover simple clock time, hidden in the hybrid Δt . Thinking, however, that Δt was itself simple clock time, he believed he had discovered some objective thing, which has been called "space-time", which is more fundamental than either space or time. That inversion of the actual situation has persisted in physics ever since.

Simplicity can be restored by ignoring Δt for kinematical purposes, and retaining it, if at all, only for the expression of electromagnetic phenomena. Whether it will be worth while to reform the electromagnetic equations remains to be seen. In their present form, though without foundation in physical conceptions, they are nevertheless not too inconvenient in application, and it may well be that Δt affords a simpler means of expression of the same facts than does Δs . But it is not profitable to speculate concerning this until we know by actual experiment how the velocity of light is related to that of its source.

It is, however, a legitimate question in the philosophy of science whether the representation of motion by any kind of space and time measurement is ultimately capable of expressing all the phenomena. There are grounds for suspecting that it is not. To begin with, the quantity, *velocity*, defined by the ratio of a space interval to a time interval, has meaning only in relation to a finite time interval. At one instant a body is at (x, t) ; at the next instant it is at $(x + \Delta x, t + \Delta t)$; and we must wait a time Δt in order to know whether Δx is zero or not. But in the case of motion in the line of sight (and if there is motion we can always suppose an observer stationed along its line) a single, ideally instantaneous, spectrum observation will show whether there is motion or not. Hence it is possible to choose a criterion—the Doppler effect—that has a greater scope for representing motion than has 'velocity', and it may well be that in the last refinements this greater scope will be necessary for the full expression of all the phenomena presented by motion.

But there is a more general consideration pointing in the same

direction. Motion is a *relation*, and a relation has a significance over and above anything that can be expressed in terms of the relata taken separately. We know very well how the Aristotelian logic, being limited to subject-predicate propositions, is impotent to deal completely with relations. We must therefore be prepared to discover that an expression of motion which is entirely dependent on the division of the phenomenon into the separate behaviour of two bodies distinguished by their location in space, may find that some aspects of motion are beyond its power to describe. This stage, indeed, we have already reached in the Einstein conception, in which both space and time locations are relative. That conception limits us to *point-events* as the 'atomic' terms for the expression of phenomena. Consider two bodies, A and B, which move apart and then move together again. We may certainly hold that there is an event which we describe as that of the reversal of motion from recession to approach, but Einstein's theory does not allow its existence to be acknowledged. There is the event of B attaining its maximum distance from A, and the event of A attaining its maximum distance from B, but these are two different events, occurring at different places and at different times in the coordinate system in which either is at rest. There is no event of the reversal of motion. The ballistic theory, with its universal time, does not suffer from this disability; it can contemplate events covering a finite region of space; and since we can always choose to express our observations in terms of Δs , whatever experiment may show be to its relation to the readings of standard clocks, we may expect this expression to outlive that which employs Δt .

It would be of considerable philosophical interest to express the phenomena of motion in the language of the calculus of relations. So far as I know, this has never been attempted. For practical purposes, since all our observations must be made in relation to ourselves, it may well be that we shall always find an expression, in which our unique position is acknowledged, to be the one most directly related to experience, but the most comprehensive description of nature may nevertheless be possible only in purely relational terms. In any case it is unsatisfactory that, as in Einstein's theory, the concepts chosen to facilitate the study of the experience of motion should assume control and impose limitations on what they were intended merely to describe.

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GILBERT AND THE HISTORIANS (II) *

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IN the first part of this article I tried to show how some historical interpretations have distorted Gilbert's work because of their implicit inductivist assumption that it is possible to distinguish between bare experimental facts which are indubitable, and theories and speculations which are put forward to explain the facts. I suggested that Popper's thesis, namely, that there are no privileged observation statements upon which scientific theory can be inductively based, is amply illustrated by the details of Gilbert's work. It is therefore unprofitable to contrast, as many historians have attempted to do, Gilbert the experimenter with Gilbert the speculative theorist, for Gilbert's theories determine throughout the interpretations he makes of his experiments.

In considering Gilbert's theories and speculations, however, another interesting question arises, connected with the second main point of Popper's thesis. Where Gilbert explicitly puts forward theoretical explanations, is it possible to distinguish between those of his theories which are genuinely falsifiable, and those which are meta-physical in the sense of Popper's falsifiability criterion? There are two reasons for asking this question. Firstly, it leads to an assessment of Gilbert's theories on essentially different grounds from those of the inductivist historians, and also, secondly, it throws some light on the historical importance of the criterion itself, for it forms part of a general inquiry into the extent to which acceptance of the criterion constitutes the seventeenth-century revolution.

13 *Gilbert's magnetic forms*

I shall not deal here with Gilbert's cosmological speculations or their relation to the Copernican system, but shall concentrate on those of his theoretical explanations which might be expected to come closest to the subsequent practice of science, namely those in electricity and magnetism where his experimental work is strongest. This is not to

* The first part of this paper appeared in the previous Number.

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say that his cosmology was unimportant, but it does not show so clearly the character of his 'new sort of philosophizing' based on the experimental method. Gilbert himself makes this point, for he remarks that in Book VI (on cosmology), it is permitted to 'philosophize freely' (Preface to the Reader).

Gilbert appears to be as confident as Bacon in the ability of experiments to demonstrate true causes:

But after the magnetic nature shall have been disclosed by the discourse that is to follow, and perfected by our labours and experiments, then will the hidden and abstruse causes of so great an effect stand out, sure, proven, displayed and demonstrated; and at the same time all darkness will disappear, and all error will be torn up by the roots and will lie unheeded; and the foundations of a grand magnetic philosophy which have been laid will appear anew, so that high intellects may be no further mocked by idle opinions. (7)¹

It is noticeable, however, that he makes this claim only in the case of his theory of magnetism, and indeed his theory of electricity is much less remarkable. The single chapter on electric attraction to which I have already alluded is a model of experimental investigation of traditional theories, but Gilbert's own theory in this case is hardly different in kind from those he has refuted, and not much more capable of development. He has no difficulty in disposing of some of the explanations of electric and magnetic attraction suggested by the Greeks and his Renaissance predecessors, such as that similars attract, and the attraction is by heat (as in evaporation), or by suction of a vacuum, or by air motions; but in the case of electricity, he considers, as we have seen, that the electric body is alone active and does not change the nature of the bodies which are drawn, and he finds that the attractive effect is screened off by dense media. He therefore concludes that the effect is due to some kind of material emanation from the electric. He suggests an analogy to explain its mode of action. Electric bodies are of a watery rather than an earthy nature, and the humour which they emit has a tendency to union and continuity like the cohesive tendency of water drops, and so it allures light bodies towards itself. Magnetic attraction on the other hand is not affected by the intervening medium:

But if indeed these things resulted from a material ingression, then if strong and dense and thick substances had been interposed between the

¹ Page references in brackets are to *On the Magnet*, ed. D. J. Price, New York, 1958.

bodies, or if magnetical substances had been inclosed in the centres of the most solid and the densest bodies, the iron particles would not have suffered anything from the lodestone. But none the less they strive to come together and are changed [i.e. magnetised]. Therefore there is no such conception and origin of the magnetic powers. (66)

This conclusion leads Gilbert to the theory of magnetic forms which has been described pejoratively as metaphysical and animistic. Before considering this assessment we must discuss what Gilbert means by 'form'

In the early part of the book, forms appear to be Aristotelian: they are qualities of bodies or properties of a species, with the added assumption that bodies having the same form, as iron and lodestone, will 'conform' to each other, or affect each other in regular ways. But in distinguishing electric from magnetic effects Gilbert says:

In all bodies in the world two causes or principles have been laid down, from which the bodies themselves were produced, matter and form. Electrical motions become strong from matter, but magnetic from form chiefly; and they differ widely from one another and turn out unlike, since the one is ennobled by numerous virtues and is prepotent, the other is ignoble and of less potency. (52)

The magnetic form referred to here is not a formal cause in the Aristotelian sense, but an efficient cause of magnetic motions (64). Magnetic bodies attract by 'formal efficiencies (*formalibus efficientiis*), or rather by primary forces (*vigoribus*)':

This form is unique and peculiar, it is not what the Peripatetics call the *causa formalis*, . . . but it is the form of the primary and original globes . . . not Aristotle's primary form, but that unique form which preserves and disposes its own globe. (65)

But apart from the fact that Gilbert regards this particular form as primary and prepotent, it is not clear that he intends anything more by it than the properties or forces of magnets as observed in their motions. He thought there were five such motions: (1) coition, (2) rotation towards the poles of the earth and direction of the earth towards the poles of the universe, (3) variation of the compass from the poles of the earth's rotation, (4) declination or dip of the magnetic needle, and (5) rotation of the earth which (Gilbert thinks) is natural to all spherical magnets. All these motions are said to be caused by 'concord' or 'conformation' of magnetic bodies with the earth

and with one another, and there are many passages in which there is no need to read more into this language than Gilbert's assertion that he has discovered five distinct ways in which magnets act upon one another and described them accurately. Thus, his main theoretical assertion, as we have seen, is that the *cause* of the behaviour of magnetic needles on the surface of the earth is that the earth is a great magnet—the efficient cause or active agent is the earth, and does not lie (as had been suggested) in the sky, and the earth acts in this way because it has a form of a primary kind. When Gilbert discusses declination, he remarks that he has established the rules (*rationes*) of this motion in many striking experiments and that 'in the following pages we shall demonstrate the causes of it, in such a way that no sound, logical mind can ever rightly set at nought or disprove our chief magnetic principles'. (184). In the subsequent pages the '*causa certa et verissima*' turns out to be the conformation of the magnet to the earth (not its attraction to the earth's nearest pole), and the fact that this is an action of the whole earth (not of the earth's centre only) (187). And a diagram showing the angle of dip of needles at various points around a *terrella* is described in these words:

While some assign occult and hidden virtues of substances, others a property of matter, as the causes of the wonderful magnetic effects . . . we have laid hold of the true efficient cause, as from many other demonstrations, so also from this most certain diagram of magnetic forces effused by the form. Though this form has not been brought under any of our senses, and on that account is the less perceived by the intellect, it now appears manifest and conspicuous even to the eyes through this essential activity which proceeds from it as light from a lamp (207).

The fact that the magnet acts at a distance shows that the form is 'effused' beyond the limits of the body, but Gilbert insists that he does not mean by this that it is self-existent in space:

And yet we do not mean that the magnetic forms and orbs exist in air or water or in any medium that is not magnetical, . . . for the forms are only effused and really subsist when magnetic substances are there (205).

In this the magnetic form is like light, which 'does not remain in the air . . . and is not reflected from those spaces', but only exhibits its action when reflected from objects (77). Gilbert's analogy with light is instructive, for it shows that he does not accept the Aristotelian

and medieval conception of propagation of form through a passive material medium as an explanation of the perception of bodies at a distance or of the action of the magnet. According to the theory developed by Roger Bacon and other medieval writers, images, forms or 'species' of objects are 'multiplied' in space under the influence of illumination, and the form which is thus conveyed across space becomes embodied in the retina of the percipient. In a similar way the magnet 'multiplies its species' in space, and if there is iron in the vicinity, the species is actualised in it, causing it to become a magnet. In these theories there is no action at a distance, and the form is propagated by successively modifying portions of the material medium, as the impression of a signet ring modifies the wax. Gilbert, on the other hand, appears to accept a true action at a distance both in the case of reflected light and of magnetism. Just as the species of things appear in the eye only when both a luminous body and a reflecting object are present, he says, so the magnetic virtue appears only when two magnets are present. This interpretation of Gilbert's statements receives confirmation from the fact that, unlike Aristotle, he believes that there is void space in the heavens¹ and also that the heavenly bodies act upon one another magnetically; it therefore follows that in Gilbert's view magnetic virtue is not dependent on a medium for its propagation. 'It is in bodies themselves that acting force resides, not in spaces or intervals' (217).

14 *Cause and Explanation*

So far, Gilbert's statements about the magnetic form are of two kinds. First, the form is the cause of the observable magnetic motions in the sense that it only subsists when they are present, and it is made 'manifest and conspicuous' by them. But it is not identical with them, because a quality of primacy and prepotency is ascribed to it which is metaphysical in the sense that it does not appear to add anything to the empirical content of the form. If this is the case Gilbert has not, in the modern sense, put forward any causal *explanation* of magnetic effects by speaking of a magnetic form, and yet in some sense he does regard it as a cause.

In order to understand this sense, it is necessary to distinguish the widely accepted modern view of explanation as construction of a theory from which the phenomena to be explained and new observable

¹ *Philosophia Nova*, Amsterdam, 1651, p. 30

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phenomena are deduced, from the view of explanation as a 'making plain' by describing an unusual or unexpected or otherwise puzzling event in terms of events more familiar or more intelligible. The second kind of explanation is not sufficient for the first, and may not even be necessary, although explanation in terms of familiar models is one important way in which explanations of the first kind have been realised.

Gilbert does not use even his theory of electric attraction as an explanation of the first kind. He makes no deductions from the theory of electric humours which could be tested or lead to new discoveries, and when he comes across an apparent refutation, he modifies the theory so as to accommodate the awkward fact. For example, he refutes the assertion that electric attraction is due to the motion of air by bringing a flame up to a piece of attracting amber and showing that the flame is not disturbed. But lest it be thought that the electric effluvium which he postulates would also disturb the flame, he says that this is 'much more subtle than air' (55). Again, he shows that moisture in the atmosphere *hinders* attraction, how then can it be that a moist humour causes it? An *ad hoc* and somewhat obscure explanation is given by appealing to the behaviour of sticks partly immersed in water (56-58). The treatment of magnetic 'conformation' is equally *ad hoc*. In so far as this notion arouses any expectations about the behaviour of magnets over and above an actual description of their known motions, these expectations are surely refuted by the following experiment which Gilbert reports as something of a paradox. Suppose one end of an iron rod is touched by the north pole of a terrella and the rod then removed to a short distance. It will tend to swing with the end which has been touched pointing to the north pole of the terrella. But if a piece of the terrella itself in the shape of a rod be cut out along a magnetic meridian, and this piece removed to a short distance, it will not remain in its original orientation, as one might expect if pieces of a body 'conform' to their parent body, but will swing round to point in the opposite direction. But this is of course perfectly consistent with the rules about coition of unlike poles, and Gilbert remarks that these rules show the 'true and genuine conformation', for 'magnetic substances seek a unity as regards form; they do not so much respect their own mass' (122). In other words, they behave according to the rules described, and this is to 'conform'. The notion of conformation does not constitute a testable theory in the modern sense.

In the sense of making comparison with a more familiar process, however, Gilbert's explanation of electric attraction clearly qualifies. If one is prepared to accept the self-attractive properties of moisture as 'natural' or for some reason not demanding further explanation, and if it can be shown that an electric humour having the same properties would account for all known phenomena of electric bodies, then it may be said that electric attraction itself is explained. It was this conception of explanation of the unfamiliar in terms of the familiar that was attacked by Bacon with the comment that the familiar and ordinary must also be explained, but he so far agreed with the process exemplified by Gilbert's electric humour as to suggest that the ultimate explanation is often better sought in the ordinary than in the odd—the nature of cohesion for instance in solid bodies rather than in soap bubbles.¹

Is Gilbert's magnetic form an explanation even in this sense? What familiar process has sufficient analogy with magnetic actions to qualify as an explanation of them? Gilbert has already refuted the analogies with vacuum suction, attraction by heat, propulsion by circular air currents, and action of humid emanations, in the case of magnetism, and has decided that nothing material passes between magnets to cause their mutual actions. No action, he thinks, takes place *by means of matter* except by contact (57). But this action does not take place by means of matter: it may therefore either be an action at a distance, or a manifestation of self-movement requiring no external propulsion. What is it that manifests self-movement? Clearly the answer is that *animated* bodies do so, and so Gilbert arrives at his analogy between the magnet and self-moving souls—his so-called animism. The magnet can transmit its form to a distance without a material medium. That is a pure action-at-a-distance, and requires, Gilbert thinks, a soul of higher order even than our own (210). The form itself which is transmitted is a power of self-movement, showing itself when two magnets 'mutually agree' to conform to one another. The magnetic force

seems to be very like a soul. For the power of moving itself seems to point to a soul; and the supernal bodies . . . are thought by some to be animated, because they move with admirable order (68).

Here we have the positive analogy between the magnet and the soul—the power of self-movement, and movement according to the

¹ Bacon, *Novum Organum*, Book I, lxxxviii

order and harmony of the whole system. This movement tends to preserve the heavenly bodies 'for their own good', so that the universe is prevented from falling into 'wretchedest chaos' and the earth from being 'vacant, dead and useless' (210, 227). After full allowance has been made for Gilbert's teleological and animistic language here, it should be noticed that the chapter in which he introduces this discussion of the magnetic soul is headed 'Magnetic force is animate, or *imitates* a soul (*anima*); and in many things surpasses the human soul while this is bound up in the organic body' (208, my italics). Gilbert goes on to point out the *negative* analogy between the magnet and the soul, and although he uses this for the quasi-theological purpose of showing how much more noble are the heavenly bodies (including the earth) than human souls, nevertheless in the light of these passages he cannot be accused of making a facile identification of the heavenly forms and human souls. The negative analogy is as follows: the globes are not restricted by possession of organs, they are incorruptible and perfect, they are able to emit 'immaterial effused forms' outside the limits of their bodies, all of which distinguish them from human souls which are 'brought and carried away by a breath',¹ and finally, the globes make no mistakes in their rational and orderly movements:

But those motions in the sources of nature are not caused by thinking, by petty syllogisms, and theories, as human actions, which are wavering, imperfect, and undecided; but along with them reason, instruction, knowledge, discrimination have their origin, from which definite and determined actions arise, from the very foundations that have been laid and the very beginnings of the universe (210).

What, then, can we conclude about the status of Gilbert's 'true causes' of magnetism? First, that the magnetic form is something that manifests itself in five kinds of motion observably exhibited by magnets. Second, that the heavenly bodies are magnets in the sense of exhibiting these motions. This is a theory in principle capable of test, for the orbits of the heavenly bodies can be compared with the behaviour of experimental terrellae. But if we ask, thirdly, how the magnetic form is a theory of the *nature* of magnetism, or a *cause* of magnetism as opposed to a summary description of magnetic behaviour,

¹ Mottelay's translation of *De Magnete* is so inaccurate here as to have led at least one commentator into ascribing to Gilbert the assertion that the *immaterial effused forms* of magnets are 'brought and carried away by a breath', that is, that magnetic motions are caused by an effluvium which, because more subtle, is more 'immaterial' than matter. But when Gilbert says 'immaterial', he means 'not material'.

it must be said that it gives a causal explanation only in the sense of giving a familiar analogy, and not in the sense of being a falsifiable theory. The form is the cause of magnetic motions in the way that the soul is the cause of animal motions, but there is no pretence that this analogy can be pressed further to admit of tests and new predictions. It is metaphysical in the sense of being unfalsifiable.

15 Gilbert and Kepler

It has been claimed that Gilbert's metaphysical magnetic form initiated the idea of a field of force, but this cannot be said to be accurate, for Gilbert would certainly have rejected a statement such as Faraday's to the effect that lines of magnetic force exist as a 'condition of space free from . . . material particles'.¹ Gilbert was rather the first to discuss in detail a true action-at-a-distance, and to use the Aristotelian distinction of matter and form to help him to define it. But like many theories which with positivist purity try to avoid the assumption of unobservable interphenomena, this one led to no theoretical developments in the way Gilbert stated it. It only did so by being, perhaps deliberately, misunderstood. The misunderstanding, if it were such, was Kepler's. In his *Mysterium Cosmographicum* of 1596, Kepler is already comparing terrestrial gravity with magnetism (an identification which Gilbert never made),² and speaking of an *anima motrix* in the sun which causes the motions of the planets.³ Later (probably encouraged by *De Magnete*), he regards this latter force as magnetic in nature. He ascribes to Gilbert the doctrine that 'magnetic fibres or filaments' extend from the south to the north poles of the

¹ Faraday, *Experimental Researches*, Vol. III, p. 414

² Gilbert is still widely misinterpreted on this point. The notion that he ascribed gravity to magnetism seems to go back to Bacon, who remarks (*Novum Organum* II, xxxv) ' . . . if Gilbert's opinion be received, that the earth's magnetic power of attracting heavy bodies does not extend beyond the orb of its virtue (which acts always to a certain distance and no more) . . . ' Cf. also his fragmentary introduction to *The History of Heavy and Light Works* (ed. Ellis & Spedding, Vol. V, p. 202). Bacon has confused Gilbert's magnetic 'orb of virtue' (xvj) with his 'orb of effluvia' (229) (the atmosphere), which causes heavy bodies to descend to their parent body, the earth, and which unlike magnetic force, acts *by contact*. Hooke, following Bacon, misinterprets Gilbert in the same way: 'Gilbert began to imagine it [gravity] a magnetical attractive power' (a paper of 1666 reported in Birch: *History of the Royal Society*, London, 1756, Vol. II, p. 70).

³ Kepler, *Opera*, ed. Frisch, Vol. I, p. 174

GILBERT AND THE HISTORIANS (II)

earth,¹ and develops this in his own theory that the sun directs the planets by means of 'filaments or fibres' emitted in the zodiacal plane. But Kepler's force, unlike Gilbert's, exists in space as a 'species',² and its mode of action is nearer to the medieval 'multiplication of species' than to Gilbert's carefully defined magnetic form. And finally Kepler drops the word 'anima' and replaces it by 'vis' for a reason which shows that his conception of 'anima' is very different from that of Gilbert. He discovers that the velocity of a planet varies inversely with its distance from the sun, and this precise quantitative variation leads him to the conclusion that what is emitted from the sun must be corporeal 'at least in a certain sense'.³ Variation according to precise rule would never have led Gilbert to drop his notion of the ordering soul of the sun, for it was just this property of regularity which had suggested it to him.⁴

It must be remembered that the connotations of 'body', 'spirit', and 'soul' were very various in Renaissance writers and not clearly distinguished from each other. Distinction and clarity were to some extent introduced by the Cartesian dualism and the dogmatic mechanism of subsequent physics, with its 'spiritual effluvia', which were material. It is to Gilbert's credit that he is not trapped into mechanism as an explanation of magnetic phenomena, as were all his seventeenth-century successors until Newton, and yet at the same time he manages to avoid the vagueness of his predecessors. It is true, as Koyré remarks, that Gilbert's magnetic form could not be mathematised⁵ and so played little part in the developments which culminated in Newton's *Principia Mathematica*, but on the other hand the diagrammatic and numerical character of Gilbert's work places it firmly in the Platonic mathematical tradition.

16 Gilbert and Bacon

Finally, it is of interest to compare the roles of Gilbert and Bacon as initiators of the experimental philosophy. It has often been remarked

¹ Kepler, *Werke*, ed. Caspar, Vol. VII, p. 334

² Kepler, *De Stella Martis* (1615), *Werke*, Vol. III, pp. 246, 350

³ Kepler, Notes (1621) to *Mysterium Cosmographicum*, *Opera*, Vol. I, p. 176

⁴ Dr Agassi has rightly pointed out that in endowing magnets with souls Gilbert merely endowed them with self-movement and harmony (this *Journal*, 1958, 9, 240). Kepler's earlier view is more properly called 'animism' in the sense of the sixteenth-century cosmologies (cf. R. G. Collingwood, *Idea of Nature*, Oxford, 1945, pp. 94 f.).

⁵ Koyré, *Etudes Galiléennes*, pp. 99, 138, 148

as a puzzle¹ that although Gilbert agrees with Bacon that experiments are the true foundations of natural philosophy, Bacon does not seem over-anxious to claim him for an ally, sometimes conclusions detrimental to Bacon's character have even been drawn from this fact. But a dispassionate reading of Bacon lends no colour to the picture of him as a jealous and vindictive ignoramus, deliberately disparaging Gilbert's views and suppressing his work.² It is always preferable to find reasons based on philosophic grounds for philosophic disagreement rather than those based on personalities, and whatever may be the truth about the latter, Bacon himself gives clear indications of the philosophical reasons for his critical remarks about Gilbert's method. Apart from these criticisms, almost all his comments on Gilbert's results and opinions are either favourable or else polite expressions of dissent from Gilbert's astronomical views, for Bacon is anti-Copernican, and although his opposition here may reflect his ignorance of current astronomy, it surely cannot be ascribed to any particular antipathy to Gilbert.

The passages which are relevant to Bacon's views of Gilbert's method are those in which he inveighs against the Empirics and the philosophers who, seduced by the Idols of the Cave (that is, their own predilections), build a whole philosophy out of the results of a few experiments:

The race of chemists again out of a few experiments of the furnace have built up a fantastic philosophy, framed with reference to a few things; and Gilbert also, after he had employed himself most laboriously in the study and observation of the lodestone, proceeded at once to construct an entire system in accordance with his favourite subject.³

¹ Most recently by D. H. D. Roller: *Isis*, 1953, 44, 10

² Cf. J. Pelseneer: 'Gilbert, Bacon, Galilée, Kepler, Harvey et Descartes', *Isis*, 1932, 17, 171; Benjamin: *Intellectual Rise in Electricity*, pp. 317 f. The accusation of suppression made by Benjamin is based on the fact that the manuscript of *Philosophia Nova* was in Bacon's possession at his death, and that there are references to Gilbert's views expressed therein in Bacon's writings. But it does not follow that Bacon had any evil motives in not causing them to be published during his lifetime. It is at least as likely that he thought their publication would *damage* Gilbert's reputation (as has in fact been the case among Bacon's disciples, the inductivist historians), or at least that they do not add anything important to *De Magnete* (which has also been the subsequent general opinion).

³ Bacon, *Novum Organum*, Book I, liv. Cf. *ibid.* lxiv; *Advancement of Learning*, *Works* ed. Ellis & Spedding, Vol. III, 293. Other references to Gilbert are to be found in *Works*, Vol. III, 366; Vol. IV, 179, 218, 220, 223, 323, 360, 426; Vol. V, 202, 454, 493, 515, 518, 533, 537, 556.

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The charge is, then, that Gilbert, like the alchemists and other Empirics, performs multitudes of experiments without order and method, and then leaps to the unjustified conclusion that their results provide him with universal explanations. Since a commonly accepted view of Baconian science is precisely that it falls itself under the first part of this indictment, it is necessary first to remark that Bacon's method does not depend on any such indiscriminate gathering of facts, nor does it stop at the level of experiments, but envisages the construction of elaborate theories. His method of drawing up Tables of Presence and Absence involves selective observation and artificial experiment which is already far from the unsystematic procedure of which Bacon accuses the Empirics. But this is not the end of the matter—Bacon also has a vision of a 'Ladder of Axioms' up which the investigator may climb from phenomena to more and more general Forms, and at any stage of which he may descend to new observations.¹ It is therefore essential to Bacon's method that explanations should enable new predictions to be made,² and here Bacon's precept was undoubtedly better than Gilbert's practice. Bacon's problem in looking for Forms was a problem of fundamental physics (what he called 'metaphysics'), and in the case of magnetism it would have been the problem of the 'nature' of the phenomenon, expressed in terms of configurations of ultimate, general, elements of the world, and enabling magnetism to be linked, via the Ladder of Axioms, with other phenomena, known and yet to be discovered. In so far as Gilbert's theory of magnetism was either merely descriptive, or metaphysical in the sense of yielding no predictions, he fails to provide what Bacon demands,³ and what the subsequent history of electromagnetism has in fact provided. In other respects, however, the subsequent practice of science justifies Gilbert rather than Bacon. Where Gilbert is looking for the 'Form' (in the Baconian sense) of electric attraction, he uses a method of crucial experiment and elimination which is similar to Bacon's method, but what Gilbert eliminates are previous *theories*, not, as Bacon does, *qualities* which lie on the surface

¹ For an analysis of Bacon's philosophy of science see my 'Francis Bacon', forthcoming in *A Critical History of Western Philosophy*, ed. D. J. O'Connor.

² See especially Bacon, *Novum Organum*, Book, I, cvi.

³ Bacon does not explicitly charge Gilbert with this last offence, but it is implied in his comparisons of Gilbert with the alchemists, for of them he says that they only make new discoveries by accident, whereas Bacon's method will lead to new and useful results as consequences of the discovery of true axioms.

of phenomena and are immediately apparent to the empty mind. And Gilbert makes no attempt to show that he has eliminated all but one of the possible theories, but merely replaces a few refuted theories by another which, according to Bacon, is equally an illegitimate anticipation. Most of Gilbert's experiments do not have the form of construction of tables of presence and absence of qualities, but are designed and described in accordance with his theories, in order to develop and test his theories, and most of these theories are in the Baconian sense the wildest anticipations. Given Bacon's standpoint, his comments on Gilbert are quite intelligible, but Gilbert's hypothetical method is nearer the pattern of later physics, for the empty mind is an illusion.

Whipple Museum, Cambridge

DISCUSSIONS

THE FALSIFIABILITY OF THE LORENTZ-FITZGERALD CONTRACTION HYPOTHESIS: A REJOINDER TO PROFESSOR DINGLE

PROFESSOR Dingle's reply¹ to my refutation² of Popper's *ad hoc* charge against the original Lorentz-Fitzgerald contraction hypothesis makes the following three claims:

- (i) 'The [Lorentz-Fitzgerald] hypothesis was certainly *ad hoc*, for the realisation of its falsifiability came very much later, after it had been displaced by the special relativity theory of Einstein.' And my demonstration that the Lorentz-Fitzgerald contraction hypothesis is *not ad hoc* within the framework of the aether theory is thus indicted as being 'much more misleading than the original statement [by Popper]'.
- (ii) When, in 1903, Lorentz deduced his *pre-relativistic* transformations from two specified fundamental assumptions and then, in turn, derived the Lorentz-Fitzgerald contraction from his transformations, the contraction 'ceased to be an *ad hoc* hypothesis, and became falsifiable by anything that would falsify these two assumptions'.
- (iii) Concerning the availability of experimental evidence that might have falsified but in fact confirmed special relativity and, in particular, Einstein's assumption that the velocity of light in space is independent of the state of motion of the emitter, Dingle says the following: 'The . . . assumption . . . could be made the subject of a laboratory experiment to compare the times of arrival of pulses of light, proceeding from bodies in relative motion but emitting at the same point, at a distant point along the line of motion. This was, of course, impracticable in 1905, but it should not be beyond the power of modern techniques. It is highly desirable to test, if possible, this basic assumption of existing physical theory.'

The aim of the present note is to show that the first two of Dingle's assertions are untenable and that the statement of his plea for a laboratory test of Einstein's assumption is quite misleading as to the amount of observational evidence already available to support it.

- (1) The following two very different questions are confounded by Dingle:
- (i) Is a certain auxiliary hypothesis independently testable, in principle, within the logical framework of the theory which it modifies?, and (ii) Is the proponent of the collateral hypothesis at issue *aware* of such independent testability, if it obtains? To assert that an auxiliary hypothesis is *ad hoc* within the context of the theory to which it pertains is to attribute a certain logical property to that hypothesis in the specified theoretical framework. Dingle overlooks that the possession of this logical property by the hypothesis cannot be held to consist in the failure of its advocates to *realise* that it does, in fact, lend itself to being tested. As well say that a certain proposition which is, in fact, provable in a given axiom system can be asserted *not* to be a theorem at a certain time on the strength of the failure of all mathematicians to *see* then that its proof is feasible.

¹ This *Journal*, 1959, 10, 228

² This *Journal*, 1959, 10, 48

What Dingle does establish is the *historical* fact of Lorentz's personal methodological culpability, viz. that Lorentz espoused his contraction hypothesis in the face of his own belief that it was not susceptible to independent test. But, contrary to Dingle, this *biographical* fact does *not* show that the hypothesis was *ad hoc* prior to 1903. As I endeavoured to point out in my earlier note, the incorrectness of the *ad hoc* charge is tellingly demonstrated by the capability of the Kennedy-Thorndike experiment (and of others) to furnish an *independent test* of the Lorentz-Fitzgerald contraction hypothesis. To this Dingle replies that *awareness* of this capability did not come until well after the modified aether theory had been supplanted by Einstein's special theory of relativity. But this retort is quite irrelevant. For the refutation of the *ad hoc* charge by the Kennedy-Thorndike experiment is *internal to the aether theory* and no more depends logically on the availability of the special theory of relativity than does the refutation of the *original* aether theory by the null outcome of the Michelson-Morley experiment.

(2) Even if Dingle *were* right in maintaining that the contraction hypothesis was *ad hoc* prior to 1903, the reason given by him for the cessation of the alleged *ad hoc* status during that year does *not* entail this conclusion, as he supposes. He reaches the latter conclusion by inferring fallaciously that the hypothesis became falsifiable in 1903 in virtue of the falsifiability of the two fundamental assumptions from which Lorentz had deduced it during that year via his pre-relativistic transformation equations, a *non-sequitur* which denies the antecedent in a conditional argument.

(3) New, improved tests of a hypothesis are, of course, always a *desideratum*. But by calling for a laboratory test of Einstein's assumption that the speed of light is independent of the motion of the source *without* mentioning the already existing evidence for it, Dingle misleadingly suggests that to date this assumption is still devoid of an experimental foundation. Not only do we have corroborating evidence for it, but that evidence was sufficiently compelling to convince an investigator like R. C. Tolman,¹ who had been an exponent of the most natural rival of Einstein's assumption,² viz. of Ritz's version of the emission theory of light. According to that emission theory, the speed of light is c only relative to its source, while in any other frame K , the velocity of light and the velocity of its source are additive. And the observational findings *disconfirming* Ritz's alternative to Einstein's assumption include the following results, among others:³

(i) On Ritz's theory, the execution of the Michelson-Morley experiment with light coming *not* from a terrestrial source but from the sun should issue in a shift of the interference fringes on rotation of the apparatus. But Tomaschek reported in 1924 that he had found no such effect, and

(ii) In experiments involving *open* rather than closed light paths, the Einstein and Ritz assumptions entail differences even in regard to effects of only the first order in the quantity v/c . To be sure, no open light path experiment of the type suggested by Dingle seems to have been performed with *terrestrial* sources of light. But this fact can hardly detract from the force of the *astronomical* evidence pertaining to first

¹ Cf. R. C. Tolman, *Relativity, Thermodynamics and Cosmology*, Oxford, 1934, pp. 15-17

² Cf. R. C. Tolman, *Physical Review*, 1910, 30, 291 and 1910, 31, 26

³ For a comprehensive account, see W. Pauli, *Theory of Relativity*, London, 1958, Part I, §3, pp. 6-9 and 207; also, the reference in footnote 1 above.

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order effects which Comstock and de Sitter have marshalled against the Ritz hypothesis : the existence of distant double stars whose orbits are observed to have *small* eccentricities strongly disconfirms Ritz's postulate by showing that the speed of light could differ from c by at most only a small fraction of the velocity of the source.

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REPLY TO PROFESSOR GRÜNBAUM

If I were sure that I understood exactly what Professor Grünbaum is now saying, which I am not, I do not think I should consider that it justified protracted discussion. Nor, in fact, did I regard his original point—the question whether the Fitzgerald-Lorentz contraction hypothesis is falsifiable or not—as more than a matter of trivial detail, a question whether a particular event among many was or was not an example of a general principle. I wrote my comment because the matter seemed to me to have incidental aspects which are important at the present time, to which I thought it worth while to call attention.

But to save meanderings along unprofitable by-paths, I would now merely point out—as I should have done at the beginning if I had been discussing the matter for its own sake—that Professor Grünbaum's statement that the Kennedy-Thorndike experiment could falsify the contraction hypothesis is simply incorrect. The experiment gave a null result, which was consistent with the contraction and the 'time dilatation' operating together. If a positive result had been obtained, then, depending on its amount, it could have been deduced that the contraction alone, or contraction supplemented by a different time dilatation, had been established. No conceivable result of the experiment could have falsified the contraction hypothesis, or even increased the probability of its falsity.

With regard to the experimental evidence for Einstein's postulate of constant light velocity, Professor Grünbaum has apparently not seen a paper in *Mon. Not. R.A.S.*, 1959, **119**, 67. There is at present no experimental evidence at all either for or against this postulate.

HERBERT DINGLE

THE PARADOX OF CONFIRMATION *

HEMPEL's paradox of confirmation has recently been discussed by a number of authors¹ but I wish to offer a completely different treatment. The paradox is exemplified thus :

* I wish to thank the referee for some incisive criticisms while this note was in course of preparation.

¹ References may be traced through J. Agassi, this *Journal*, 1959, **36**, 311-317.

The hypothesis, H , that all crows are black is the same as that all non-black things are not crows, and this is supported by the observation of a white shoe. Which seems paradoxical.

I shall argue that :

(i) The observation of a white shoe *does* support H (provided that the number of non-black objects that might be observed is known to be large compared with the number of crows), and the result appears paradoxical because the support given to the hypothesis is negligible. The argument depends on the judgment that a shoe can be regarded as a typical non-crow.

(ii) More rigorously, i.e. independently of the above kind of judgment, the observation of a non-black non-crow supports H if the observation is of 'stoogian type'. The concept of the Stooge, which will be defined below, is, in my opinion fundamental in statistical practice.

(iii) When not of stoogian type the observation of a non-black non-crow does not necessarily support H , and may even undermine it to some extent if the object observed bears a strong resemblance to a crow. For example, the observation of a white raven undermines H .

The main issue will be brought out by imagining first that the only objects we can observe are crows and shoes, and the only colours black and white. These assumptions will be relaxed later.

Imagine a 2 by 2 contingency table in which the rows are labelled 'crow' and 'shoe' and the columns 'black' and 'white'. The hypothesis H states that one of the cells in this table is empty. To get an entry in the table in either of the two cells that are adjacent to the empty cell is support for the hypothesis that it is empty. If quantitative considerations are ignored then the problem is symmetrical in rows and columns, and a white shoe is as good as a black crow.

It is not strictly accurate to ignore quantitative considerations since the argument breaks down in some extreme cases. In order to make the argument quantitative it is convenient to make use of the terminology of 'odds', of 'Bayes factors', and of 'weights of evidence'. A weight of evidence is an explicatum of 'support' for a hypothesis, and it was actually called 'support' by Jeffreys.¹

If p is a probability, the corresponding odds are $p/(1-p)$, for example, a probability of $\frac{1}{2}$ is odds of 1. Odds run from 0 to infinity when p runs from 0 to 1. If H is a hypothesis and E is evidence, then $P(H|E)$ denotes the probability of H given E , and the odds of H given E are

$$O(H|E) = P(H|E)/P(\bar{H}|E),$$

where \bar{H} denotes the negation of H .

The initial probability of H is $P(H)$. Strictly there is always some initial evidence before an observation is made, but it is often convenient to take it for granted and to omit it from the notation. As a consequence of an observation that provides evidence E the final probability of H is $P(H|E)$.

The factor by which the initial odds are to be multiplied in order to get the final odds is $O(H|E)/O(H)$. It is called the 'Bayes factor' in favour of H and is equal to

¹ H. Jeffreys, 'Further Significance Tests', *Proc. Cam. Phil. Soc.*, 1936, 32, 416-445

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$P(E | H)/P(E | \bar{H})$, as may easily be deduced from the product axiom. This ratio is a simple form of the 'likelihood ratio', but the expression 'likelihood ratio' is also used in a more general sense in which it is not equal to the Bayes factor.

A Bayes factor exhausts all the information from an experiment, or from evidence, that can be relevant to the probability of H . This statement is obvious if we are allowed to talk about the probabilities of hypotheses (and if these probabilities are not zero), since the factor determines the final probability completely in terms of the initial probability. Since this statement is true whatever the initial probability it is also true even if the initial probability is unknown. If however a language is adopted in which it is forbidden to talk about the probabilities of hypotheses, then the statement can still be justified in terms of the Neyman-Pearson theory of 'errors of the first and second kinds', provided of course that the simple likelihood ratio is used instead of the Bayes factor.

The discussion of the case where $P(H) = 0$, which is regarded as important by Popper, would take us too far afield.

Let the logarithm of a factor be denoted by $W(H : E)$, or more generally by $W(H : E | G)$ when the probabilities are conditional on another proposition, G . This 'log-factor' has the additive property.

$$W(H : E \cdot F) = W(H : E) + W(H : F | E).$$

Therefore a log-factor does not merely tell us everything about the evidence that can be relevant to the probability of H ; it also has an additive property analogous to that of ordinary weights in the scale of a weighing machine. (The analogy is even stronger when E and F are statistically independent given H and also given \bar{H} , since the last term then reduces to $W(H : F)$. This happens when both H and \bar{H} are so-called 'simple statistical hypotheses'.) $W(H : E | G)$ is therefore appropriately called the 'weight of evidence', or, more fully, the weight of evidence in favour of H (against \bar{H}) provided by E given G . When we are taking for granted that one of two hypotheses is true, so that we may describe one as the negation of the other and denote them by H and \bar{H} , then the weight of evidence for H provided by E (if it can be calculated) gives a complete summary of the evidence from E . Nothing else about E can be relevant to the selection between H and \bar{H} . But the initial probability of H , and the utilities of H and \bar{H} if true, are relevant also to the choice between them. The choice should be consistent with the 'principle of rational behaviour', which is the recommendation to maximise expected utility.¹ Since E tells us nothing about the initial probabilities and utilities, it follows that the only thing in E relevant to the choice is the weight of evidence. The unit in terms of which weights of evidence are measured depends on the base of the logarithms. (Cf. the decibel.)

Now suppose that there are N objects that might be seen at any moment, of which c are crows and b are black, and that the N objects each have probability $1/N$ of being seen. Let H_i be the hypothesis that there are i white crows, and suppose that the hypotheses H_1, H_2, \dots, H_c are initially equiprobable. Then, if we happen to see a black crow, the Bayes factor in favour of H is

¹ See, for example, I. J. Good, *Probability and the Weighing of Evidence*, London, 1950, p. 40; 'Rational Decisions', *J. Roy. Statist. Soc., ser. B*, 1952, **14**, 107-114; L. J. Savage, *The Foundations of Statistics*, New York and London, 1954.

$$\frac{c}{N} \div \text{average} \left(\frac{c-1}{N}, \frac{c-2}{N}, \dots, \frac{1}{N} \right) = \frac{2c}{c-1},$$

i.e. about 2 if the number of crows in existence is known to be large. But the factor if we see a white shoe is only

$$\begin{aligned} \frac{N-b}{N} \div \text{average} \left(\frac{N-b-1}{N}, \frac{N-b-2}{N}, \dots, \max(0, \frac{N-b-c}{N}) \right) \\ = (N-b) \div \max(N-b-\tfrac{1}{2}c-\tfrac{1}{2}, \tfrac{1}{2}(N-b-1)), \end{aligned}$$

and this exceeds unity by only about $\frac{1}{2}c/(N-b)$ if $N-b$ is large compared with c . Thus the weight of evidence for H provided by the sight of a white shoe is positive, but is small if the number of crows is known to be small compared with the number of non-black objects.

Similar results can be obtained if the two assumptions of equiprobability are dropped, although the algebra is more complicated.

The argument can be generalised still further by allowing N , c , and b , themselves to have probability distributions, not necessarily known to us. The same conclusions will follow if N is finite, say less than the exponential of the number of particles in the observable universe. The qualitative results are distribution free if $N-b$ is assumed to be certainly (or even only highly probably) large compared with c ; and they are independent of whether we use a theory of personal (subjective) probability or of 'credibility' (logical probability).¹ If $N-b$ is not probably large compared with c the argument breaks down. For example, suppose that all objects ever seen in the past were black. Then the observation of a white shoe would undermine the hypothesis H .

In order to generalise from 'white shoes' to 'non-black non-crows', I introduce the Stooge. A stooge will be understood to be a man who must carry out an observation and then reply 'yes' or 'no' to the questions 'was it black?' and 'was it a crow?' If he gives any other information he will be shot dead and knows it. Thus an observation of stoogian type in effect lumps together white shoes, green pebbles, and horses of all colours except black ones. For stoogian observations the previous argument applies with the rows of the contingency table labelled 'crow' and 'non-crow', and the columns 'black' and 'non-black'. Therefore a stoogian observation of a non-black non-crow supports H .

The use of a stoogian observation is equivalent to a decision to avoid certain types of information on grounds of simplicity and objectivity. An example is the decision to avert our gaze from the particular choice of random numbers in a randomised statistical design, in order that we should be able to make an objective statement of significance. Another example is the decision in law to ignore the evidence of a copy of a copy. The verbal simplification of evidence by ignoring some of it is indeed so familiar in ordinary life that it becomes a habit. Whenever we communicate our observations to others we have continually to judge what it is fair to omit. Omission is usually a lesser distortion than insertion, and in stoogian observations the decision what to omit is made in advance, so that personal bias is minimised. (Perhaps this is the justification of those forms of cross-examination in which the person being interrogated is expected to answer the question asked, without much elaboration.)

¹ For a recent article on kinds of probability see I. J. Good, *Science*, 1959, **129**, 443-447.

THE PARADOX OF CONFIRMATION

The purpose of introducing stooges in the present discussion is much the same as in other contexts, namely that it simplifies the theory. We can first prove our thesis rigorously under the limitation that the observation is stoogian. Having done this we must go on to consider what modifications are required if the observation is not stoogian.

The paradox can be stated in the form "the proposition 'I have just seen a non-black non-crow' supports H ". Although this form is true it does not follow that if someone correctly says "I have just seen a non-black non-crow" that H is supported, for he may be maliciously suppressing some relevant information. But when a Stooge expresses a proposition by means of a remark, the fact that he has made the remark conveys the same information about H as does the proposition expressed. For Stooges do not maliciously suppress information.

If, after a stoogian observation is made, we happen to acquire the suppressed information, we have to judge whether this information should cause us to revise our conclusions. (For example, we might discover that all our random numbers were zeros.) A white shoe seems to me to be a typical non-black non-crow, so that in this case it makes little difference whether the observation is stoogian. But a raven is not a typical example of a non-crow, and it is intuitively obvious that the non-stoogian observation of a white raven would undermine the hypothesis that all crows are black.

The argument concerning the white raven can also be made quantitative by thinking in terms of a contingency table, this time 2 by 3. The columns may be labelled 'black' and 'white' as before. The rows may be labelled 'crows', 'ravens', and 'other things'. We may denote by $H_{i,j}$ the hypothesis that there are i white crows and j white ravens. Any reasonable initial probability distribution for the $H_{i,j}$'s should incorporate the assumption that i and j are correlated, since crows and ravens belong to the same family of birds. Under a wide class of reasonable assumptions it will be found that the observation of a white raven undermines the hypothesis that all crows are black.

For a stoogian observation it is unnecessary to think in terms of a contingency table of dimensions more than 2 by 2.

The justification for the three assertions listed at the beginning of this note now seems to me to be adequate.

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ERRATA

In the May No., 1960, p. iii, 'Mr B. Braithwaite' should, of course, have been 'Professor R. B. Braithwaite'.

In the errata of the same No., p. 88, a mistake has been made. The following appeared: "p. 315 insert, on the left hand side of the table of possibilities at the bottom of the page, ' $\omega =$ ' before ' $\sim e$ ', so that ' $\omega = e$ ' stands before the lowest bracket." Of course the end of the sentence should have read: "' $\omega = \sim e$ ' stands before the lowest bracket."

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Towards a Unified Cosmology. By Reginald O. Kapp.
Hutchison & Co., London, 1960. Pp. 303. 35s.

SCIENTISTS and philosophers often find it difficult to agree as to what constitutes the 'philosophy of science'; and indeed it has been said that this subject is frequently neither good philosophy nor good science. Such a criticism could not be levelled at this book by Professor Kapp. *Towards a Unified Cosmology*, as its title implies, is concerned to develop a general theory of Cosmology and to examine its consequences; and the general theory is arrived at by the rigorous application, to possible hypotheses of the past and future extension in time of the material universe, of what is called 'The Hypothesis of Minimum Assumption'.

The Hypothesis of Minimum Assumption is, in effect, Ockham's Razor, or the rule of economy of hypotheses; and it is Professor Kapp's contention that this is, in fact, the most basic of all the principles of physics. According to him the laws of physics are not restrictive in the sense in which laws in statute books are. They do not set out to prohibit any particular event, but allow everything, which is consistent with observable facts, to occur. This, it is suggested, means that every valid generalisation in physics can be stated so that the terms 'any' and or 'either' occur in its formulation. Progress towards unification in physics has been most rapid when physicists have acted on this assumption; and when specific assumptions have been made these have always had to be abandoned in favour of non-specific generalisations of the type defined above. The notion that valid generalisation in physics involves the use of the words 'either' or 'any' is formally stated as 'The Principle of Minimum Assumption'.

So far Professor Kapp has been concerned with establishing the philosophical bridgehead from which further advances can be made. Having stated 'The Principle of Minimum Assumption', and described its status and analysed its philosophical basis, he then proceeds to apply it rigorously to the various possible hypotheses about the past and future duration in time of the material universe. There are three possible hypotheses about the duration of matter and energy in the past: that they have existed for an infinite time; that they have existed for a finite time; and that they have existed for any period of time. There are three corresponding hypotheses about the future duration of matter and energy: that they will exist for an infinite time; that they will exist for a finite time; and that they will

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exist for any period of time. There are thus nine possible combinations of these hypotheses, each of which can be used as the basis for the development of a cosmological model. It will be noted, however, that only one of them contains the word 'any' in both its constituent hypotheses. Application of 'The Principle of Minimum Assumption', therefore, leads Professor Kapp to select this as the only valid generalisation. This philosophically derived hypotheses about the duration of matter and energy in the Universe is stated as follows :

Any particle of matter or quantum of energy may have existed for any length of time . . . matter and energy are originating without cause, continuously, at random, and not as a result of anything in the existing state of affairs.

Any particle of matter or quantum of energy may cease to exist at any time . . . matter and energy are disappearing without cause, continuously, and by extinction, at random, and not as a result of anything in the existing state of affairs.

This amounts to saying that matter and energy are being continuously created and destroyed, a view first advanced by Professor Kapp some thirty years ago. It is interesting to note that some twelve years ago Bondi and Gold independently arrived at the first part of this hypothesis, although for different reasons, which they published as the Theory of Continuous Creation.

The notion of continuous creation and destruction is called the Hypothesis of the Symmetrical Impermanence of Matter; and it obviously satisfies both the criteria embodied in Ockham's Razor and Professor Kapp's definition of a valid generalisation. Scientific hypotheses, however, must not only conform to philosophical criteria, they must also be predicative. In an observational, as opposed to an experimental, study the predicative capacity of a theory is investigated by using the theory to produce a model—in this case a cosmological model. The various properties of the model are then compared in detail with the observed activity in the external Universe; and one's attitude to the theory is then determined by the degree of correspondence exhibited in this comparison.

The cosmological model which is constructed on the basis of the Hypothesis of the Symmetrical Impermanence of Matter has many interesting features. The origin and evolution of the Galaxies is explained in terms familiar to those who have read the theory of continuous creation of Hoyle, Bondi, and Gold. Kapp shows that according to his theory one would expect that new clouds of matter would form in extragalactic space at finite intervals of time, and that their evolution into spiral nebulae should occur during a fairly short period of time during this evolution. The formation of the spiral arms should be accompanied by turbulence in a very large quantity of extremely tenuous gas. Although one would not expect this

to lead to the emission of visible radiation it might cause radio-frequency emission. One might, therefore, expect regions in the neighbourhood of extragalactic nebulae to emit radiation which could be received by radio-telescope ; and such observation would be valuable confirmation of this particular cosmological model.

The most unexpected consequences of the Hypothesis of the Symmetrical Impermanence of Matter, however, are obviously those which follow from the hypothesis of the continuous destruction of matter. This leads to some unexpected conclusions. Kapp begins this section of the work by considering the various meanings of the word ' mass '. He distinguishes three meanings: inert mass; attracted (weight) mass; and attracting mass. He goes on to say that General Relativity Theory is based on the identity of inert mass and weight mass but that no satisfactory explanation of attracting mass has ever been given, beyond the statement that a particle carries associated with it an extensive gravitational potential gradient. Einstein pointed out that in regions of space curvature one can infer that a body free from restraint and possessing inert (and hence ' weight ') mass is accelerated. Now if the body is near an accumulation of inert mass it is observed to be accelerated; and therefore one assumes that space, in the vicinity of an accumulation of inert mass, is curved. This does not show that it is in the nature of inert mass to *cause* curvature of space but merely that it is in the nature of inert mass to *follow* curvature. General Relativity Theory goes no further than this; and hence in the past we have merely been able to say that every particle possessing inert mass has associated with it some curvature of space.

If, however, we assume that whenever a particle is created some space is also created, and that similarly when a particle becomes extinct, some space also becomes extinct we may make the following inference. During a particle's continued existence it has no attracting mass, but only inert and weight mass. When it becomes extinct, a local contraction of space occurs which manifests itself as a local curvature of space. This local contraction does not remain stationary, but travels outwards as a pulse ; this constitutes a gravitational field; and thus gravitation is quantised and has a finite velocity of propagation. Furthermore, since the total number of particles becoming extinct in any given period of time in any assemblage is a function only of the total number of particles present two further conclusions follow. The strength of the gravitational field associated with a given number of particles is proportional to that number, and hence bears a constant ratio to the inert and weight mass of the assemblage; and also one may talk about the half-life of matter in the same way in which one discusses the half-life of radio-active elements.

Professor Kapp arrives at a value of about 4×10^8 years for the half-life of matter. If this is so it means that the mass of the Earth and the other planets has been continuously decreasing since the time of their formation.

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From this follow a number of remarkable astronomical, geological, and biological implications ; and one fancies that not all of these will be readily accepted by exponents of these particular disciplines.

One further consequence of the hypothesis that space is intensely curved in the neighbourhood of very dense aggregates of matter is that in the vicinity of atomic nuclei, gravitational forces are sufficient to account for the cohesion of nuclei. It is thus no longer necessary to postulate additional short-range nuclear forces.

The above inadequate resume is sufficient to show that Professor Kapp has produced a very remarkable book, of a type which is all too rarely written. It is essentially readable; it contains very little mathematics; and as has already been pointed out it is concerned in a very proper way with the philosophy of science. One is particularly impressed by the organisation of the material, the cogency of the arguments and the lucidity of the presentation. This book should be read by all those scientists who are concerned with the philosophical bases and status of their subjects and by philosophers who are interested in the development and meaning of scientific theories. Not everyone will agree with all of Professor Kapp's conclusions; but those who disagree will be stimulated by the ideas put forward and one suspects that the author hopes that this may happen. For in this way progress in generalisation in physics will occur; and on this road, 'Towards a Unified Cosmology' will be a very significant milestone.

H. D. TURNER

Modern Philosophy of Science: Selected Essays. By Hans Reichenbach.
Translated and edited by Maria Reichenbach. Foreword by Rudolf Carnap.
Routledge & Kegan Paul, London; Humanities Press, New York,
1959. Pp. x + 214. 28s.

THIS fair sample of the almost two hundred writings of Reichenbach (1891-1953) has been edited by his wife, who is to be thanked for her careful work. It is useful both as an introduction to Reichenbach's thought and as a document for tracing the origins of most of his later, more elaborate, publications. In a single compact, well printed and bound volume, we can contemplate the birth of ideas which are more difficult to grasp when fully developed and couched in a highly technical and personal language.

The main subjects discussed in the eight essays contained in this volume are: relativity, causality, probability, and ethics. The first essay, written in 1921, is a somewhat aged discussion of the criticisms raised against relativity by fictionists, Machians, and neo-Kantians; it also expounds Schlick's and Reichenbach's own views, which were definitely favourable to Einstein's

theories. One wonders whether at that time Reichenbach (who had just published his first book on relativity) clearly grasped the physical ideas he dealt with: in fact, the paper—which is cogent on the philosophical side—contains several mistakes. For example, the contention that in general relativity only coincidences are invariant (a basic condition for an operationalistic interpretation), whereas the metrical relations between the coincidences are relative—as if the line-element and the differential equations were not invariant, and as if they made reference to coincidences. Another mistake is the assertion that the Lorentz formulae express the causal relationships between the observations in different systems: only their empirical test requires a translation into an observational language, but even so co-ordinates do not acquire a causal efficacy. Also misleading is the assertion that the geocentric and the heliocentric descriptions of planetary motions are equally correct, a contention repeated by Reichenbach in later works; there is no such equivalence, among other reasons because the principle of equivalence is a differential law claiming no validity in extended regions of space-time. A further mistake is the statement that, for velocities faster than light, the kinetic energy becomes infinite: actually, it becomes complex. The philosophical discussions are, however, enlightening.

Reichenbach's probabilistic conception of causality, discussed in three of the present essays, rests on the remark that the statement of any (quantitative) law of nature, whether causal or not, should be supplemented by an hypothesis about the probable error—whence it is inferred that 'probability laws and causal laws are logical variations of one and the same type of regularity'. This view overlooks the peculiar logical status of statistical laws, which are general but not universal, and the semantical gap separating law statements from statements about law statements (e.g. 'Law L is exact to within 1 per cent'). Whereas law statements refer to things and processes, statements about laws and their test or application belong to a different level. Causation and chance are certainly not external to each other, and one should sympathise with attempts at disclosing their relations, but such attempts require a deeper logical (and ontological) analysis. The same is true of determinism, which Reichenbach equates with predictability, an identity which enables him to dispose easily with the former, since no completely exact quantitative predictions are possible. But determinism, an ontological theory, cannot be equated with predictability, a human capability, among other reasons because there are as many kinds of predictions as kinds of laws—in particular, it is possible to predict the evolution of frequency distributions.

Similar criticisms may be raised against Reichenbach's marriage of induction and probability: namely, that such a union is effected by a definitional stipulation. Induction 'is at the core of the theory of probability', as Reichenbach maintains, if and only if an inductive definition of

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probability is chosen—for instance, as the limit of relative frequency. But what if the technical objections of the mathematicians are taken into account and if an implicit definition is adopted, in which the term ‘frequency’ does not occur—as is the case with Kolmogoroff’s and Popper’s theories of probability? Induction does occur, not however in the mathematical theory itself (which is formal) but in some of its applications—as, e.g., when we speak of (observed) stable frequencies in the long run. A further logical weakness is found in the very use of the term ‘probability law’ (for statistical law): it should be applied only to the axioms and theorems of the theory of probability, since ‘probability’ designates a mathematical, not an empirical concept. Again, the ambiguous term ‘probability statement’ should be used with care: one should distinguish between the frequency of collections of events, the probability of single events, and the probability of statements. The penalty for overlooking these distinctions is Reichenbach’s proposal of the adoption of many-valued logic, on the ground that ‘probability statements are not meaningful within a two-valued logic that requires every statement to be either true or false’.

The essays on ethics, which were written in 1951 and 1952 and are published posthumously, are probably the most interesting of all. They show us a rigorous and vigorous philosopher who has overcome some of the taboos of his youth to the point of dealing seriously with the freedom of the will. Freedom of action is characterised by certain probability implications (among them, ‘The probability that in a situation A, a volition VB will lead to the action B, equals approximately one’) supplemented with certain requirements (e.g. the causal relevance of the volition to the action). The probability conditionals are then interpreted as causal conditionals contrary to fact, and finally the following definition is reached: ‘An action is free if it would have been influenced by a preceding volition’. The reviewer does not know of a more adequate logical analysis of the *potestas agendi*. The treatment of free will, on the other hand, does not seem to be as satisfactory. The definition of free will is just a specialisation of that of free action: ‘A volition is free if it would have been influenced by a preceding volition’. The interpretation of freedom of the will as self-determination is dismissed, on the ground that the term ‘internal cause’ is vague—as if ‘volition’, a primitive in this study, were less vague. Finally, the cognitive explication of ethical discourse is discussed. While accepting that a set of valuations is involved in the taking of any given course, and that the knowledge of those valuations takes part in conscious action, Reichenbach is concerned with showing that the transition from the cognitive conclusion ‘The action A will supply maximal satisfaction of the set of values V’ to the action A is not a cognitive affair, since one might refuse to do A. He espouses the non-cognitive (Humean) theory of ethics, according to which

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imperatives are not derivable from declarative statements. While the core of this theory is true and important, it still remains a negative theory that does not satisfy our longing for a rational and empirical foundation for ethical 'utterances': that the Stoics failed in this enterprise should be regarded as a challenge rather than as a demonstration of the impossibility of giving ethics a scientific foundation.

The volume ends with a useful exhaustive Bibliography including Reichenbach's early articles on youth and students' problems, and which suggests that there is room for another volume containing some of his scattered essays on logic and logical analysis. For Reichenbach's most solid contributions, apart from *The Philosophy of Space and Time* and from his various *apologiae pro philosophia scientifica*, seem to be his logical works, which are not marred by a phenomenalist epistemology and an inductivist methodology.

MARIO BUNGE

The Way Things Are. By P. W. Bridgman.

Harvard University Press & Oxford University Press, 1959. Pp. x + 333, 45s.

LOOKING back on a double career as a Nobel Prize physicist and an amateur philosopher of great influence, Professor Bridgman is well entitled to air his views on Things in General.

Any man's personal wisdom is bound to be idiosyncratic (and Bridgman was never one to balk at idiosyncrasy), and so the value of the book to a reader will depend largely on his prior acquaintance, and sympathy, with Bridgman's views. To the unsympathetic it will appear as something of a mixed bag, containing very sharp insights together with reflections appearing naive at least. Arranged in one reviewer's rough order of increasing quality, there is the theory that a graduated income tax is the first step to Communism, the disbelief in the actual infinity of aggregates, the conviction that physics took a wrong turning with Planck's solution of the radiation problem, thought-experiments on relativity (a rotating searchlight whose beam transverses a distant nebula at more than the speed of light), a critique of the 'atomic' ideal in non-physical sciences, and a discussion of the meaning of 'event' in a physical system without particles.

Bridgman feels that there is an idea underlying all these various reflections: that we can't get away from ourselves, while physics generally proceeds on the assumption that we can. This may indeed be the case, and it would be valuable to see the idea applied as a tool of philosophical analysis, so that its strengths and limitations could be revealed. This is what Bridgman did with the 'operational' idea in his *Logic of Modern Physics*, and the positive

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residue of the insight has passed into the stock of common assumptions of modern philosophy of science. But in the present book, the basic insight is only a starting point for personal reflections on 'the way things are'—and so the appreciation and evaluation of the work is, unfortunately, a matter of taste.

J. R. RAVETZ

Physics in my Generation. By Max Born.

Pergamon Press, London and New York, 1956. Pp. 232 + vii. 40s.

TWENTIETH-century physics rests on principles established in earlier centuries. But it has rejected three principles which as late as 1900 seemed unlikely ever to be displaced, namely determinism, absolute space and absolute time. There were preliminary indications of doubt, but in each case there was one penetrating and dramatic analysis which marked the beginning of the new mode of thinking. For determinism this was Max Born's paper 'On the Quantum Mechanics of Collision Processes'.¹ For absolute space and absolute time it was Einstein's paper 'On the Electrodynamics of Moving Bodies'² which, by fusing them into an absolute space-time, was decisive.

Indeed with the exception of Einstein and possibly Bohr no physicist in this century has contributed more than Max Born to the reassessment of the problem of explanation in physics. For the philosopher of science, Born's contribution to the twentieth-century revolution has been more radical than Einstein's, and (perhaps for just this reason) has taken longer to attract the attention of non-physicists. It has also not been quite so clear-cut. For this two reasons may be adduced. In the first place, Bohr had already presented thirteen years earlier a theory of atomic spectra in which determinism was provisionally suspended. Bohr's theory was logically incomplete in that while using Newton's mechanics to describe the motion of electrons inside atoms, together with a new principle to select only discrete orbits as possible in nature, it ascribed radiation to an unanalysed 'transition' process, in which determinism played no part. A logically self-contained theory emerged in 1925 and 1926 from the fusion of two apparently unrelated, but in fact equivalent approaches. The first, Heisenberg's abstract matrix mechanics, in which Born (whose assistant Heisenberg was) had played a prominent part, was at first difficult to interpret physically. The second was Schrödinger's wave mechanics, which its author hoped would establish atomic behaviour as an undulatory motion of a continuous medium. During this period, development in the application of 'quantum mechanics'

¹ Max Born, *Z. f. Physik*, 1926, 37, 863

² Einstein, *Annalen der Physik*, 1905, 17, 891

(a name coined by Born) was so rapid, that the decisive overthrow of determinism in Born's paper, in which he showed that Schrödinger's wave mechanics could be used to explain collision processes, and that this required the wave-function to be interpreted as determining a probability, and not (as Schrödinger had believed) a continuous density, was not immediately differentiated from the wealth of other new and valuable results whose philosophical importance was less, nor recognised as the decisive step it really was. In the second place, with characteristic modesty, Born, while giving the clearest reasons why determinism probably had to be abandoned, and showing that in its non-deterministic form the theory was at least logically self-contained, left the possibility open that someone more penetrating than himself might yet formulate the theory in a deterministic way.

To those familiar with the intimate history of physics in the first half of this century much of this will emerge from between the lines of *Physics in my Generation* in which Professor Born collects together his popular writings over a period of thirty years. But Professor Born is not good at blowing his own trumpet, and is uneasy when he is dealing with just those branches of physics where his own contributions have been the greatest. He is at his best when analysing the thought of his friend Einstein, for here no feeling of modesty is present to restrain him; and his own deep insight into physics illuminates the workings of the creative process which he understands so well.

The articles gathered together here are of several kinds: prefaces and postscripts to books, popular lectures, semi-popular review articles. Not all are concerned with the philosophy of science. Some, towards the end (the order is strictly chronological) are concerned with the moral issues raised by nuclear weapons, and by the pursuit of objective truth for its own sake. The most substantial, however, are concerned with the rôle of theory in physical science.

Many good physicists are uneasy when trying to analyse their activities from a philosophical standpoint, and in spite of his success at it, I suspect that Born is often among their number. Preoccupation with the philosophy of a science requires the cultivation of the significant generalisation, but every generalisation involves definition and delimitation, and the latter can be the enemy of scientific progress. Occasionally one glimpses reminders in these essays that all is not as in the writings on 'scientific method'. For example (p. 30):

At first, however, they (the formulations of quantum mechanics) were only formalisms, and it was a matter of discovering their meaning *a posteriori*. It is in fact, very common in physical investigations to find it easier to derive a formal relation from extensive observational material than to understand its real significance. The reason for this lies deep in the nature of physical experience:

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the world of physical objects lies outside the realm of the senses and of observation, which only border on it; and it is difficult to illuminate the interior of an extensive region from its boundaries.

Occasionally, too, Born's generalisations, condensed into a pithy sentence, raise disagreement or doubt, but challenge one to look at some of the deepest problems. For instance (p. 125):

He (Bohr) stressed, from the very beginning, the new features of the scheme, namely the indeterministic character of the transitions, the appearance of chance in the elementary processes. This means the end of the sharp separation of the object observed and the subject observing. For chance can be understood only in regard to expectation of a subject.

The last sentence gives me a jolt. I do not know whether I agree with it or not, but I am forcibly reminded that behind the quantum theoretical requirements on the concept of probability, to be defined even for a single elementary (and possibly quite unrepeatable) event, there lies a problem of logic whose solution I do not know. Or again (p. 89):

I cannot see how the Bose-Einstein counting of equally probable cases can be justified without the conceptions of quantum mechanics.

Here I personally disagree with the implication, but once again I am jolted into looking critically at an important principle.

I was impressed by the concept of 'style' in science, which Born takes over from Pauli in the Guthrie Lecture (pp. 123-138), and again in his rejoinder to Schrödinger's 'Are There Quantum Jumps?' (p. 150). Each age of science has its characteristic style, and the style of our century is quite distinctive. And if one wants to learn to appreciate this style, short of contributing oneself to its creation one can hardly do better than to read *Physics in my Generation*.

M. H. L. PRYCE

A History of Science. Vol. 2: Hellenistic Science and Culture in the Last Three Centuries B.C. By George Sarton.

Harvard University Press; London: Oxford University Press, 1959.
Pp. xxvi + 554. 63s.

THE late George Sarton devoted his life to the study and dissemination of the history of science. *Isis*, the review he founded in 1913, is now the leading journal in this field; the critical bibliographies which he instituted as an annual feature of *Isis* have been of inestimable value to scholars in many areas; his *Science and the New Humanism* is still the most eloquent plea for the unification of Snow's two cultures; the monumental five volume *Introduction to the History of Science* is indispensable to the serious student of ancient and medieval science.

In his later years, Professor Sarton saw the necessity of completing his scholarly career by weaving together the many strands of his researches into a single synthesis. His hope was to produce a history of science in eight volumes in which the evolution of scientific ideas from antiquity to the present day would be traced in detail. When he died in 1956, however, he had just completed the present volume, and it must constantly be kept in mind that he was unable to see this work through the press. Some of the short-comings noticed below might have been eliminated had Sarton lived.

The volume is divided into two parts: the first deals with the third century B.C., the 'Alexandrian Renaissance'; the second with the last two centuries before Christ, when the pall of Rome was slowly being cast over the Mediterranean world. In each part, Sarton sets the scene with a description and analysis of the social and cultural milieu in which the scientific achievements took place. Chapters are then devoted either to the work of men like Euclid and Archimedes or to general fields such as geography, anatomy, natural history, and medicine. To complete the map of the intellectual world, there are chapters on philosophy and religion, history, philology, literature and the arts.

The work as a whole is very uneven and suffers considerably from the fact that Sarton's aim far exceeds his reach. That he was correct in insisting that the history of science must be written within the larger framework of cultural history cannot be denied; but to do this, the author must be able both to understand and explain the scientific aspects of a given period, and to see the past in historical perspective. There can be no doubt about Sarton's scientific competence but his view of history is almost painfully naïve. The *dramatis personae* are divided into 'good guys' and 'bad guys', and customs into 'good practises' (writing plain, rather than effusive, dedications; supporting science) and 'bad practises' (slavery and oriental superstitions). From this clash of black and white, one supposes, comes the dynamic of history.

In the non-scientific chapters, Sarton carries his documentation to an extreme. Must we be informed by a footnote that Hannibal 'was the greatest Carthaginian general' (p. 13, footnote 28), or that the correct pronunciation of Japan's largest city is 'Tōkyō' (p. 9, footnote 19)? Is it essential that we be told that the popularity of the Winged Victory and the Venus de Milo is partially due to their being in the Louvre, and then exhorted to 'remember that a great many works of art have been shown in the Louvre for centuries without becoming popular' (p. 510, footnote 27)? A 'footnote frequency' of four per page, consisting in large part of such trivia, surely can be described only as pedantry.

Finally, these chapters suffer from two closely allied faults which arise from Sarton's laudable desire to present a complete picture of the Hellenistic intellectual world. On the one hand, he is led into relatively lengthy

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digressions which serve no useful purpose and will confuse or bore many readers whose main interest happens to be the history of ancient science. Thus the discussion of the *Milindapanha*, an Indian dialogue between King Milinda (a Greek) and a Buddhist priest, displays Sarton's admittedly vast learning but detracts from his narrative. In other instances, he falls back on a general catalogue of names, dates and works which could easily be found in a classical dictionary. In a book written expressly for 'men of science who are anxious to know the origins of their knowledge' (p. xi), what are we to make of the following (complete) entry?

Philodemos of Gadara was an Epicurean poet, a contemporary of Cicero. His poems (some thirty) were eventually included in the *Crown*, not in the first edition, but in the second, prepared by Philippos of Thessalonice (c. A.D. 40) (p. 460).

Since fourteen of the twenty-nine chapters deal with non-scientific matters, these criticisms cannot simply be dismissed as referring to areas peripheral to the central theme of the work.

In his discussion of the state and development of science in the Hellenistic period, Sarton is on much firmer ground. The union of his unsurpassed bibliographical talents and his profound knowledge of ancient science provides unique chapters in which the reader not only finds the body of ancient science revealed, but also has the road of transmission from antiquity through Byzantium, Islam, and medieval Spain and Sicily mapped for him.

Curiously enough, these chapters are open to the criticism that they fail to deal with ancient science in sufficient detail. Does not Archimedes' proof of the law of the lever, which figured so prominently in the development of statics and, with Mach, in the history of the philosophy of science, deserve reproduction here? Should not the *Conics* of Apollonios be given greater space? Raised on analytical geometry and the calculus, how many of us, without special training, could handle the problems treated in that work? How much real understanding of higher Greek mathematics can we have if we are told only that the ellipse, the hyperbola, and the parabola can be dealt with by geometric methods? How tantalising to be informed that Book V of the *Conics*, 'Maxima and Minima', was Apollonios' masterpiece and never be given even a hint as to how Maxima and Minima, without the differential calculus, could be determined!

It is not the province of a reviewer to suggest the kind of book an author should have written. In this case, however, I cannot help but wish that Sarton had cast his net less widely. Had he dealt with Hellenistic science in more detail and allowed his vast erudition to embellish rather than overpower this tale, there can be little doubt that this volume would have been the best memorial to a life spent in the service of scholarship.

L. PEARCE WILLIAMS

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The Science of Mechanics in the Middle Ages, 1200-1400. By Marshall Clagett.
University of Wisconsin Press, 1959. Pp. xxix + 711. \$8.00.

'HISTORY', wrote Lord Bolingbroke, 'is philosophy teaching by example'. Like most epigrams, this one has been extensively quoted and only rarely applied. Yet, in certain areas, this approach can be utilised to great advantage both to the historian and to his audience. Consider, for a moment, the plight of the historian of medieval science. His raw material consists of texts of considerable philosophical difficulty, in many cases containing complex and ingenious mathematical and physical reasonings and written in languages which few people today command well enough to follow the substance of the argument. Moreover, he is flanked by the holders of two opposing prejudices. There are still those who insist that medieval science was mere philosophical vapourings concerned either with the number of angels who could dance on pin heads or, as 'applied philosophy', with the magical essences of gems, fabulous beasts, and so on. At the other extreme are the followers of Pierre Duhem who view modern science as a rather extended footnote to the medieval achievement. How can the historian of medieval science present his subject in such a way that he can be analytical and, at the same time, avoid the charge of prejudice or textual misreadings from his critics?

Bolingbroke provides one solution and it is this method which Professor Marshall Clagett has adopted in this latest volume of the University of Wisconsin Publications in Medieval Science. The whole area of medieval mechanics is examined in what, to my mind, is an exemplary fashion. There are four parts: Medieval Statics, Medieval Kinematics, Medieval Dynamics, and The Fate and Scope of Medieval Mechanics. With the exception of Part IV, each section consists of a number of analytical chapters in which Professor Clagett discusses with great skill the development of various aspects of medieval mechanics, and then illustrates these developments with extensive selections from medieval documents. In Part IV, Professor Clagett sums up the preceding 625 pages in two excellent chapters. The first, 'The Reception and Spread of the English and French Physics, 1350-1600', is an admirably balanced account of the fate of the ideas developed by the Schoolmen at Merton College and Paris during the thirteenth and fourteenth centuries. The final chapter, 'Medieval Mechanics in Retrospect' considers the contribution of medieval man to the science of mechanics. In these two chapters Professor Clagett covers much the same ground as Duhem in his three volume *Etudes sur Léonard de Vinci* (Paris, 1906-13) and Anneliese Maier in her five volumes on medieval mechanics. Duhem's somewhat over-enthusiastic claims for medieval science are corrected and Dr Maier's views nicely summarised. For the student who desires a precise evaluation of the debt of classical mechanics to the Middle Ages and who

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does not care to work his way through Dr Maier's excellent volumes, these two chapters of a little over fifty pages may be recommended without reservation.

To a large extent, the faults of this volume are the direct result of its virtues. Being partly a volume of sources and partly a history of medieval mechanics, it ends up being wholly neither one nor the other. It is certainly more than a source-book of medieval mechanics; but, because of the inclusion of sources, Professor Clagett necessarily must spend part of his time discussing their origins and these discussions sometimes seem to be more detailed than one would think necessary. The hybrid nature of the work is not a very serious fault, however, and Professor Clagett's forthcoming history of Greek science in the Middle Ages should provide the continuity sometimes missing here. This volume is clearly the foundation on which Professor Clagett intends to build and, as such, it could hardly be better.

This is a handsome book, well-printed and well bound. There is an excellent bibliography and index. The price (\$8.00—£2 17s. 2d.) is well worth paying for a work which will reward those who have an interest in the history of science and the history of medieval thought.

L. PEARCE WILLIAMS

Gödel's Proof. By Ernest Nagel and James R. Newman.

New York University Press, 1958. Pp. 118. \$1.75

IN one of the most significant mathematical papers of the past forty years Kurt Gödel published in 1931 a proof that the consistency of arithmetic, and hence of the whole structure of mathematics which has been built on it, is incapable of proof by methods which are susceptible of formalisation within arithmetic itself. In the same paper Gödel also showed that no formalisation of arithmetic can contain the whole of arithmetic in the sense that within *every* formalisation there are propositions $P(n)$ such that each of $P(0), P(1), P(2), \dots$ is provable but the universal proposition $(\forall n) P(n)$ is not provable. As was subsequently (and independently) shown by Th. Skolem this means that every codification of arithmetic admits a non-standard model, i.e. an interpretation in which a class of objects other than the natural numbers plays the number rôle.

The present account by Nagel and Newman concentrates upon explaining the background notions like consistency and formal proof; there is also a sketch of a method of numbering formulae which is used in the Gödel proof and a hint of one of the central ideas of the proof. But all non-technical expositions of Gödel's work cannot take the reader to the heart of the matter, which concerns the *nature* of the arithmetical functions which represent the meta-mathematical properties *via* the Gödel numbering.

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The importance of Gödel's result for the philosophy of science is in its clarification of the notion of a formal system, and the limitations of the axiomatic method. Gödel's discoveries, however, are not in conflict with views about the nature of mathematical entities radically different from Gödel's own views. Gödel sees mathematics as something which exists in advance of its discovery, so that a formalisation of arithmetic is a shadow to be compared with the real thing—arithmetic itself. But those who hold that, by and large, mathematicians are inventors, not discoverers, do not feel driven either to naïve realism or to Platonic idealism to account for the fact that a game with strict rules is not identical with one less sharply delimited.

R. L. GOODSTEIN

Induction and Hypothesis. By S. F. Barker.

Cornell University Press; London: Oxford University Press, 1957.
Pp. xvi + 203. 22s.

MR BARKER has written one of the most carefully argued and conclusive cases that I have seen against all attempts to justify non-demonstrative scientific inference in terms of any type of induction, confirmation, or formal hypothetico-deductive method. He will not, however, like this as a conclusion of his argument, for he ends by proposing yet another modification of the formalist point of view. But before coming to that, it must be said at once that he has provided an excellent summary, in non-technical language, of the pros and cons of most of the main logical theories of non-demonstrative inference. From this point of view the book would make a useful text book in a field where good introductory works are rare.

Starting with the very reasonable postulate that since we do in fact make choices between rival empirical hypotheses on the basis of evidence, there must be in some sense rules of choice between them, Barker first clears the ground by discussing the kind of hypothesis and the kind of evidence he intends to be concerned with. With regard to laws or theories, it is not to be demanded that they carry any necessity; and with regard to observation statements, no decision is to be made as to what kind of terms they should contain, that is, whether they should be sense-data or physical-object statements, or whether their truth or falsity should be assumed to be certainly knowable. It is demanded only that they be singular existential statements such as sometimes admit of verification by observation. In other words, the starting-point is consistent with most forms of empiricism.

The author then considers eliminative induction (Keynes), and enumerative induction, including Carnap's system, and rejects these, both for internal reasons and for the general reason that no such theory of induction can

account for our preference for simple quantitative laws, nor for the confirmation of hypotheses concerning unobserved entities, or containing theoretical terms (these latter, however, he later finds reasons for excluding from the class of significant scientific terms). There follow criticisms of logical constructionism and of formalism as accounts of these kinds of hypotheses, and the author then passes to the method of hypothesis not based on induction, and considers the difficulties of Popper's view that the acceptability of hypotheses should depend on their falsifiability. Finally, Kemeny's measure of the number of ways in which statements of a hypothesis in a given language could come out true in an n -member universe is used to suggest a method of comparing the simplicity of hypotheses, and a criterion of choice between two hypotheses is derived. A notable omission is any discussion of Braithwaite's account of choice between statistical hypotheses in terms of policies designed to maximise gains or minimise losses, or the analogue of the theory of games.

The author's claim for his own suggested method is a modest one, namely that it allows some simple kinds of hypotheses to gain confirmation on the basis of favourable evidence, and that conflicting hypotheses may be compared in relation to a given body of evidence. But one of his conclusions is that his method implies that no unobservable predicates ought to be introduced into theories, since predicates 'which have not been observed to have any instances' cannot increase the simplicity of a hypothesis in his sense. As he quotes as examples of such predicates 'atoms', 'electrons', and 'genes', it seems, first, that this conclusion constitutes a serious objection to his method, and second, that he has departed from his initial intention not to stipulate what kinds of terms ought to be allowed to enter observation statements. It is really not very helpful to be told, after thirty years of operational analysis, that

any extralogical term legitimately being used in science . . . in principle ought somehow to admit of being defined by means of observational predicates which do occur in the evidence. Such a definition may be quite complex and difficult to supply . . . a formidable task . . . (p. 198).

The author claims that his proposal is not reductionism, because although he does not allow for the occurrence in theories of unobserved *predicates*, he does allow for unobserved *entities* having observable predicates, for example, an unobserved planet detected by its observable gravitational effects. This is not, however, much of a concession from the point of view of theoretical science, for even ordinary discourse cannot do without it, and the peculiarly scientific problem (at least as it has appeared to formalist philosophers) is not that of unobserved entities but of unobserved predicates.

If, in spite of its rigour and clarity, one puts this book down with a feeling of disappointment, it is surely because one is reminded of the fate of the astronomy of epicycles. Formalist philosophies of science which cannot,

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for all their logical ingenuity, give a more realistic account of scientific reasoning than this, will surely pass into the same limbo, assisted thither, I am afraid, by some of the destructive arguments of this book.

M. B. HESSE

Science and the Idea of God. By C. A. Coulson.

University Press, Cambridge, 1958. Pp. vi + 51. 4s. 6d.

The Pious Scientist. By James K. Feibleman.

Bookman, New York, 1958. Pp. 111. \$3.00

THE first of these slim volumes represents the eleventh of the Eddington Memorial Lectures.

Like several others in this distinguished series it takes Eddington's ideas as a starting point rather than a theme, and the ground covered will in some measure be familiar to readers of Professor Coulson's other religious works. What is new in it, however, is not without interest for philosophers of science. Coulson is convinced (with reason, it would seem) that arguments from the *substance* of science cannot validly support belief in God. But where Eddington could speak of the 'security of our relationship with God' as sufficient to 'turn aside . . . the most convincing disproof' Coulson is not satisfied. 'In order to find God' a scientist must look, not indeed to the substance, but 'to the manner of (his) inquiry, the presuppositions which inform it, the corporate nature which it shares and his own human reactions and response to what he finds within it'.

There are acknowledged similarities in thought here to Polanyi's great thesis, particularly as developed later in his *Personal Knowledge*, and this whole line of thought deserves more attention than it has generally yet received. What may be questioned, however—and not only by unbelievers—is the extent to which the 'faith' and other attitudinal characteristics of a scientist can bear the specifically religious weight that Coulson would hang on them. Given Christian commitment, one's scientific presuppositions and attitudes do indeed find religious sanction; but is there enough (or even anything) in scientific experience as such to impel to Christian commitment? Not, indeed, 'unerringly', says Coulson (p. 49); but rather in the way that the clues in a detective story point to the criminal—becoming compelling only when the plot is uncovered. The analogy is appealing; but alas, even a full sketch of the theistic dénouement apparently fails in practice to convince some scientists that the 'clues' in their discipline have been fairly planted; and since Christ himself seems to have indicated a more operationally satisfying line of enquiry (*John*, 7: 17) they may well be right.

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As an essay in the integration of faith and thought, however, this is a notable and thought-provoking booklet, rich in apt quotation, and informed throughout with that robust yet poetically sensitive commonsense for which the author is beloved by those who know him.

Professor Feibleman is Chairman of the Department of Philosophy at Tulane University. His theme: 'An uncommitted inquiry into the nature of the unaffiliated truth, that is what is wanted' (p. 8). A sample from his 'New Creed': 'Avoid harm to others; Be altruistic to stones; Stay on the positive side'. The relevance of this book to the philosophy of science is mainly cautionary.

D. M. MacKAY

Turning Points in Physics. A Series of Lectures given at Oxford University in Trinity Term, 1958. R. J. Blin-Stoyle, D. ter Haar, K. Mendelssohn, G. Temple, F. Waismann, and D. H. Wilkinson, with an introduction by A. C. Crombie.

North-Holland Publishing Company, Amsterdam, 1959. Pp. 190. 21s.

THE essays making up this book are taken from the lectures given to Oxford undergraduates, by a group of five physicists and one philosopher. Dr Crombie's only explicit contribution is a brief introduction, but the essays speak for his skill in selecting and 'briefing' his speakers. The bulk of the book is devoted to explaining, to non-specialists, the conceptual and philosophical developments associated with the revolution in physics which took place in the first half of this century.

I shall now examine the book on an unfair basis, and see what it has to offer to readers of this *Journal*, who may be assumed to have some acquaintance with the material discussed. Historical essays such as those written in this collection by the physicists are more valuable as documents of the reflections of first-rate scientists on the history and philosophy of their subject, than as sources for the history itself. Still, they are useful summaries of developments of a period to which the professional historians have not yet turned their attention. Dr Mendelssohn's essay serves as a forceful reminder of how fortunate is contemporary physics; in a conceptual revolution where common ideas of space, time, and individuality are thrown overboard, a confused and paradoxical notion of probability somehow serves to carry an enormous burden of physical theory built upon it. Professor Temple's essay is the most stimulating and charming, probably because he concentrates on a conceptual, rather than a historical, analysis of relativity.

Dr Waismann's long essay throws light on various parts of the long and complicated history of such concepts as 'causality' and 'determinism'.

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One becomes so well convinced of the complexity of these notions, that one cannot feel comfortable with the statement of their destruction by a single formula (Heisenberg's) and its interpretations. Although Dr Waismann's examples of wave and quantum effects are most beautifully done, the reviewer has reservations about the accuracy and adequacy of some of the examples used in the discussion of classical physics. A given (finite) sequence of discrete events (number-pairs) does not need Fourier's Series for a 'law' (p. 99)—a polynomial will do nicely, and will be no more or less trivial from the theoretical point of view. The dramatic example of the ball on the round billiards table (with periodic path if the angle of impact is, in radians, rational, and with non-periodic path otherwise) is not well suited for the moral that predictability depends on complete accuracy of initial information. To determine whether a quantity is rational or irrational is not an operation of physics; moreover, to determine whether any given path is periodic is impossible in a bounded time-interval. The example shows neatly the great difference between mathematics and physics: the mathematical realisation of the initial mental image turns out to have quite interesting properties which have no physical meaning; the physical realisation might also have interesting properties (in this case it hasn't), but the mathematical formulation of those particular properties would quite possibly be of trivial mathematical interest at best.

So much of these first five essays is devoted to showing students that the physical world is a queer place, that the history inevitably takes on the tinge of showing how wrong was all physics before 1905. And the von Neumann proof makes our present conceptions as near to synthetic *a priori* as is respectable nowadays. (The historian remembers that Zeno's two-pronged proof of the impossibility of a consistent description of space and time has not yet been refuted to the satisfaction of all interested parties.) This hardening of the metaphysical arteries is characteristic of completed revolutions, as Waismann himself points out with reference to Newtonian mechanics (p. 98). The other sign that the revolution is completed is the content of Professor Wilkinson's exciting article on elementary particle physics. Yesterday's paradoxes are today's truisms (don't expect to extrapolate successfully from the macroscopic to the microscopic world); yesterday's theories give no guidance for making sense of the mass of current experimental evidence. Now one must classify, and use heuristic principles (symmetries and conservations) for making shrewd guesses. As this sort of work continues, it may be that the relevant history will cease to be the reflections of Newton and Maxwell on the foundations of mechanics, but rather the work on airs, inflammable, fixed, phlogisticated, and respirable, of Scheele, Priestley, and Lavoisier.

J. R. RAVETZ

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Revelation through Reason. By Errol E. Harris.

Allen and Unwin, London, 1959. Pp. 123. 15s.

THE surprising thesis of this essay is that sciences and religion are identical. Both are seeking knowledge of the final truth about the universe, and truth is all-inclusive and ultimately one. 'Religion . . . is the name for one's total conscious attitude toward life, as it is formed and enlightened by rational awareness and knowledge. Science in the narrower sense is an integral and indispensable factor in this, and in the wider sense which includes philosophy it is identical with it.'

According to Professor Harris, the world view of modern science, in all branches, is evolutionary. This is the picture of an ascending scale of forms, reaching completion in a reality than which nothing greater can be conceived. This leads to a reformulation of the theistic Proofs. The following form of the ontological argument is defended: nothing that is developing can be understood without references to its final state, and the final state of everything is God. Therefore God is the presupposition of all rational discourse. Therefore all putative disproofs of God's existence are self-contradictory.

Two comments: (a) The thesis of the identity of science-cum-philosophy and religion rests on a definition apparently picked for its blur-value. (b) The premiss for an evolutionary cosmology is that since the middle of the nineteenth century the idea of evolution has dominated scientific thought in all fields and guided almost every scientific investigation. This does not seem a strong basis for so far-reaching a generalisation.

R. J. SPILSBURY

ANNOUNCEMENT

BRITISH SOCIETY FOR THE PHILOSOPHY OF SCIENCE

Fifth Annual Conference : 23rd-25th September 1960

A conference will be held at Wills Hall, Bristol, from Friday evening to Sunday afternoon, 23rd to 25th September. The Conference fee will be £3 15s. 6d. for full residence and gratuities. This includes 10s. registration fee.

The programme will be as follows :

- Friday evening : 'The Relation between Pure and Applied Mathematics', Professor S. Körner and Professor R. L. Goodstein.
Saturday morning : 'Universality and Necessity', Professor W. C. Kneale and Dr I. Lakatos.
Saturday evening : 'Learning to Perceive', Mrs M. L. J. Abercrombie and Dr M. Pirenne.
Sunday morning : 'The Relation between Methods of Investigation and the Characteristics of Physical Laws', Dr D. Bohm and Dr P. W. Higgs.

Those wishing to attend are asked to write to the Hon. Secretary, Dr M. B. Hesse, Whipple Museum, Free School Lane, Cambridge.

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Dialectica, 1958, 12, Nos. 3/4

R. Péter, 'Graphschemata und rekursive Funktionen'

Kaluznin, in describing graph-schemata which could be used as definitions of arithmetical functions, suggested that if the functions were classified according to the complexity of the corresponding graph-schemata, it might be possible to give the intermediate levels for special recursive functions and general recursive functions. This paper shows that this is not possible.

A. Robinson, 'Relative Model-completeness and the Elimination of Quantifiers'

Most of the early proofs of the decidability or completeness of certain mathematical theories were based on the method of eliminations of quantifiers. Various more recent results on completeness were obtained independently of such procedures. However, it is shown in the present paper that, conversely, the completeness of a mathematical theory will in certain circumstances entail the existence of an elimination method. The proof involves the application of the extended first ϵ -theorem of Hilbert-Bernays.

H. A. Schmidt, 'Über einige neuere Untersuchungen zur Modalitätenlogik'

In the present report on some papers of the author and on a sequel to them by G. Emde, Marburg, a series of results concerning the combination of the basic modalities 'possibility' and 'necessity' are treated. Starting from some very general framing-codifications, the list of the finitely many implicative modal logics with idempotent 'possibility' which is obtainable through basis reduction is discussed. Besides basis reduction, in the case of some important subclasses of the not-necessarily-idempotent implicative modal logics, attention is given to the special decision problems.

K. Schütte, 'Aussagenlogische Grundeigenschaften formaler Systeme'

In a formal system without types which contains no contradictions and is able to represent all notions of classical mathematics, all the laws of classical propositional calculus cannot be valid. From this fact ensues the problem generally to explore the properties of formal systems from the point of view of the propositional calculus. Under certain assumptions concerning the propositional expressions the 'propositional completeness' and the 'propositional consistency' of a formal system is characterised through the *tertium non datur*, respectively through the inference rule of the *ex falso quodlibet*. There are decision procedures for the syntactical inference rules which in every formal system are valid under the given assumption and for the syntactical inference rules of every formal system which is complete or consistent with respect to the propositional calculus.

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E. Specker, 'Dualität'

The axiom system of plane projective geometry is dual in the sense that it is transformed into itself by exchange of the notions 'point' and 'line'. It follows that for every theorem the dual sentence is also a theorem. However, from the duality of the axiom system one cannot conclude that in a model the truth of a sentence implies that of the dual sentence; even less can one conclude that each model admits a 1-1 transformation interchanging points and lines and preserving the incidence relation. For projective geometry, models of this kind are well known. For the simple theory of types (where duality is replaced by ambiguity of types) it is shown that the existence of such models is equivalent to the consistency of 'New Foundations'.

Additional remark. The following theorem answers both of the questions proposed in the paper: If it is complete, then a theory with an automorphism has a model with a corresponding automorphism. NF is therefore consistent if simple theory of type with the additional axioms $S S^*$ (in the notation of the paper) is consistent.

W. Ackermann, 'Über die Beziehung zwischen strikter und strenger Implikation'

In this paper the author enters into the particulars of the relations of the concept of 'strict' implication introduced by C. I. Lewis with the concept of 'strenger' implication introduced by himself. He points out that within the system of 'strenger' implication a further concept of implication may be defined which has all the qualities of strict implication. The definition is the following: A implies B in the sense if the conjunction of A and non-B is impossible, which is not the same as the 'strenger' implication between A and B. The system of strict implication is taken in the form as formerly given by Arnold Schmidt.

E. W. Beth, "Cogito ergo sum"—raisonnement ou intuition'

According to Descartes, the demonstrative power of an argument may result either from the application of the universal rules of logic or from a particular intuition. This doctrine permits us to avoid the reproach of circularity which is often raised as an objection to certain argumentations in epistemology. On the other hand, it implies the rejection of the method of the counter-example. The acceptance of certain argumentations based on a particular intuition never creates a permanent situation; it rather constitutes a stage in the genesis of what Bernays has characterised as an *évidence acquise*.

R. Carnap, 'Beobachtungssprache und theoretische Sprache'

Among the non-logical constants of the language of science two kinds are distinguished, the observation terms (e.g. 'blue') and the theoretical terms (e.g. 'electric field'). The latter terms are introduced, not by definitions, but by postulates of two kinds, theoretical postulates, e.g. basic laws of physics, and correspondence postulates which connect the theoretical terms with observation terms. As Hilbert has explained, both mathematics and theoretical physics can in this way be constructed in the form of uninterrupted calculi. It is here briefly indicated that by this method of construction also the mathematical terms have meanings (in a wider sense) assigned to them. The theoretical terms obtain at least an incomplete interpretation by means of the correspondence postulates. It is shown how the distinction between analytic and synthetic sentences can be defined also for the theoretical language.

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H. Wang, 'Eighty Years of Foundational Studies'

A survey is made of work since 1879 on foundational problems viewed as an analysis, by reduction and formalisation, of the concepts proof, feasible, number, set, and constructivity. It is suggested that there are five domains of concepts and methods, viz. anthropologism, finitism, intuitionism, predicativism, and platonism. It is also suggested that the central problem is to characterise these domains by formalisation and to determine their interrelations by different forms of reduction. Finally, the range of logic in the narrower sense is discussed, and applications of mathematical logic are briefly outlined.

H. B. Curry, 'Calculuses and Formal Systems'

Lorenzen, in his book *Einführung in die operative Logik und Mathematik* has given a relatively precise form of syntactical system which he calls a calculus. The present paper deals with the relationship of Lorenzen's notion of calculus with the notion of formal system. It is shown that the 'obs' of a formal system can be represented as the theses of a calculus of a certain type just when the calculus has a property called the tectonic property, and conditions are given under which one form of system can be transformed into the other.

K. Gödel, 'Über eine bisher noch nicht benützte Erweiterung des finiten Standpunktes'

P. Bernays has pointed out that, in order to prove the consistency of classical number theory, it is necessary to extend Hilbert's finitary standpoint by admitting certain abstract concepts in addition to the combinatorial concepts referring to symbols. The abstract concepts that so far have been used for this purpose are those of the constructive theory of ordinals and those of intuitionistic logic. It is shown that the concept of a computable function of finite simple type over the integers can be used instead, where no other procedures of constructing such functions are necessary except simple recursion by an integral variable and substitution of functions in each other (starting with trivial functions).

R. L. Goodstein, 'On the Mature Mathematical Systems'

The crux of the dispute between formalism and intuitionism, it is held, is not whether certain entities exist or not, but how the term function shall be used in mathematics. The identification of effective definition with general recursion fails because an undefined function lies concealed beneath the requirement of a finite number of substitutions, and a fresh characterisation of effective definition is sought in terms of a hierarchy of ordinal recursions.

A correspondence exists between primitive recursive properties and direct proofs, of irrationality and transcendence for instance, and between general recursive properties and indirect proofs.

Mathematics is a concept creating activity and the distinction between a formal mathematics devoid of meaning, at one level, and a meaningful metamathematics at the next is considered to untenable.

H. Hermes, 'Zum Einfachheitsprinzip in der Wahrscheinlichkeitsrechnung'

Shimony, Lehman, and Kemeny recently developed a foundation of the theory of confirmation by reduction to the concept of rational betting. This procedure

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yields essentially only the axioms first stated by Kolmogoroff. It is well known that these are not sufficient for application. Thus it is necessary to search for a new principle if one wants to motivate new axioms. This can be done by a principle of simplicity, which expresses that the probability of a hypothesis increases with its degree of simplicity. A critical survey is given about several attempts which have been tried in Munster especially by Kiesow and W. Oberschelp with the aim to make the notion of simplicity precise. The simplicity is reduced to syntactical properties of proposition.

A. Heyting, 'Blick von der intuitionistischen Warte'

The paper contains remarks on intuitionism and its relations with other domains of foundational research. Inside the intuitionistic mathematics, in connection with Griss's criticism against the use of negation, different degrees of evidence are distinguished, depending upon the way in which conditioned constructions are admitted. Some difficulties in the theory of finite species are discussed. Concerning the foundational research in general it is observed that it has separated intuitive, formal and platonistic constituents in classical mathematics. Some remarks are made on Church's thesis in the theory of recursive functions.

G. Kreisel, 'Hilbert's Programme'

Hilbert's plan for understanding the concept of infinity required the elimination of non-finitist machinery from proofs of finitist assertions. The failure of the original plan leads to a hierarchy of progressively less elementary, but still constructive methods instead of finitist ones (modified Hilbert programme). A mathematical proof of this failure requires a definition of 'finitist'. The paper sketches the three principal methods for the syntactic analysis of non-constructive mathematics, the resulting consistency proofs and constructive interpretations, modelled on Herbrand's theorem, and their mathematical and logical consequences. A characterisation of finitist proofs is sketched. A remark on the completeness of the predicate calculus concludes the paper. Throughout open problems and alternative approaches are emphasised.

ANNOUNCEMENT

Xth INTERNATIONAL CONGRESS OF THE HISTORY OF SCIENCE

The Xth International Congress of the History of Science will be held in the United States of America from 25th August to 2nd September 1962. Opening sessions of the Congress will be held at Cornell University, Ithaca, New York, and the concluding sessions will be held at the American Philosophical Society, Philadelphia, Pennsylvania. The President of the Congress is Professor Henry Guerlac of Cornell University. The Secretary of the Congress is Professor C. Doris Hellman.

All inquiries should be addressed to The Secretary, Xth International Congress of the History of Science, Cornell University, Ithaca, New York, U.S.A. Those wishing to receive bulletins concerning the congress are requested to communicate with the Secretary.

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